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EDWARD A. TIRYAKIAN is professor of sociology at Duke University. He was chairman of the department from 1969 to 1972. A theorist, he is particularly concerned with relating changes in social structure to changes in forms of social consciousness. His publications include *Sociologism and Existentialism* (1962) and *The Phenomenon of Sociology* (1971) which he edited.

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MURRAY WEBSTER, JR., is assistant professor of Social Relations at Johns Hopkins University. With Sobieszek, he has just completed a research monograph on sources of self-evaluation. His current research involves developing information-processing models for sociological theory construction, and, with Doris R. Entwisle, applying formal "expectation states theory" to improve grade school children's self-concept.

LYNNE ROBERTS is assistant professor of Social Welfare at the University of California, Berkeley (on leave at the University of Washington, Seattle, 1972–73). With Richard Ofshe, she has published a research monograph reporting tests of utility-maximizing decision models, as well as other work in mathematical sociology and methodology.

BARBARA I. SOBIESZEK is assistant professor of sociology at the University of Rochester. In addition to experimental studies of self-evaluation with Webster, she has published work on sex-role socialization with John Meyer, and currently is studying the interaction effects of sex as a diffuse status characteristic.

SEYMOUR SPILERMAN is associate professor of sociology at the University of Wisconsin and is currently engaged in an evaluation of the New Jersey Negative Income Tax Experiment.

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STEPHEN D. WEBB is a senior lecturer in sociology at the Victoria University of Wellington, New Zealand. He is continuing his research into various aspects of industrial diversification as well as having recently begun an extensive examination of the individual and structural consequences of population density and crowding.

JAMES D. MILEY is assistant professor of sociology at Kansas State University. His major interests are theory construction, suicide, and the sociology of the future.

MICHAEL MICKLIN is associate professor of sociology at Tulane University. His major interests are Latin American demography and medical sociology.

Information for Contributors

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Davis, K 1963*a* "The Theory of Change and Response in Modern Demographic History." *Population Index* 29 (October): 345–66.

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Toward the Sociology of Esoteric Culture[1]

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The point of departure of this paper is an examination of the sociological literature on the recent occult revival in modern societies. This phenomenon, which clashes with the image of secularization, is particularly notable among the youth of the counterculture. To further sociological analysis, a conceptualization of esoteric culture is proposed. It is further argued that esoteric culture has played a significant role in Western cultural change, in such areas as artistic expressions of reality, political ideas, and even scientific thought. Esoteric culture is thus treated as a source of ideational innovations in Western modernization.

Among other bewildering aspects of the kaleidoscopic cultural scene of Western societies in recent years is a complex of phenomena which, for lack of a more precise label, has generally come to be designated as the "occult revival." Receiving public exposure in this context have been a variety of forms of popular entertainment dealing with occult themes, for example, the musical *Hair* with its "Age of Aquarius" hit song, movies such as *Rosemary's Baby* and *The Mephisto Waltz*, and the television series "Bewitched." Clairvoyants such as Jeanne Dixon in the United States and Madame Soleil in France have become public figures and authors of best sellers, while self-designated practitioners of witchcraft or even devil worship (as in the respective cases of Sybil Leek and Anton LaVey) have gotten public exposure in the mass media, along with attention to the macabre Tate-Manson affair and other cases of purported ritual homicides and suicides in which figured weird occult themes. Bookstores specializing in occult works of various kinds, frequently located in proximity to college campuses, are flourishing; sales of items bearing a Zodiac sign are booming and have even received in France, since 1969, a quasi-official blessing in the form of a special monthly drawing of the National Lottery. Tottering ivory towers have been no refuge from the occult revival: seminars and courses, with or without credit, have taken up the new "in" study, with the logical extreme being the recent formation of an "Aquarian University" in Maryland, offering a full range of esoteric studies from alchemy to Zen Buddism.

[1] Revision of a paper presented at the 1971 meetings of the American Sociological Association. I wish to thank Josefina Tiryakian for assistance in the preparation of the manuscript.

The sociological response to this new cultural development is just beginning to emerge. One of the aims of this paper is to see what sociological interpretations of this phenomenon have been recently advanced. At the same time, we will seek to carry the discussion further by outlining how the present occult revival poses broader considerations for sociological inquiry, regardless of whether the revival is destined to be a short-term affair of no significance or one with more durability. What we shall seek to develop in these pages, then, is an initial formulation of the sociology of esoteric culture and its relation to the larger social context.

THE SOCIOLOGY OF THE OCCULT REVIVAL

One of the earliest and most extensive sociological treatments of the occult revival is that of Marcello Truzzi (1970), providing, in particular, much information on the spread and organization of witchcraft in contemporary urban American society. The perspective within which the materials are presented is that of the sociology of popular culture, with modern occultism taking on the significance of a "pop religion" (we might even say of coven adepts, "Have broomstick, will travel!"), which Truzzi regards as a "demystification process of what were once fearful and threatening cultural elements." Persons playing the role of witches, for example, are attacking some of the last cultural frontiers of Western psychic inhibitions; to engage in role taking of parts formerly publicly branded as odious and the object of extreme social repression is, in a sense, to demonstrate the final liberation of Western man (and woman) from traditional cultural prohibitions dealing with the supernatural. The occult revival, at least in terms of the receptivity of witchcraft among segments of the middle class, could thus be seen as another step in the modernization of Western society, in this context as a secularization of the demonic. Such a perspective would be consonant with the secularization hypothesis concerning the relation of religion to modern society (Wilson 1966).[2]

Supplementing Truzzi's linking of witchcraft to popular and mass culture, Marty (1970) has examined a variety of publications dealing with astrology and psychic phenomena, on the basis of which he differentiates an occult establishment, responsible for most of the widely circulated publications in this area, from an occult underground press. Marty gives most of his attention to the former and notes that it is predominantly intended for an audience of middle America. In this literature there is an absence of a social message, so that "like some forms of conservative orthodoxy, the occult establishment concentrates almost entirely on individual life and often on 'other-worldly concerns'" (Marty 1970, p. 228).

[2] For a recent treatment of the secularization thesis, see Robertson (1971)

Evaluations contrasting with the above have been entertained by Staude (1970) and Greeley (1970*a*, 1970*b*). Staude views the current interest of youth in occult practices and mysticism as essentially a search for meaning and identity because "they feel alienated and disillusioned with the liberal progressivist ideology of their parents and with totalistic ideologies" (1970, p. 13); in this respect, he suggests, today's cultural setting is similar to the religious and cultural renewal of the Renaissance. More directly than Staude, Greeley has seen the import of current youth interest in occult behavior in its implications for the sociology of religion. Whereas Marty (1970, p. 228) had pointed to an absence of a communal impulse in the literature of the occult establishment, Greeley stresses such an impulse in the broad neosacral movement of today: "The young people who are involved do . . . assert that their sacred, or mystical, or occult interests do indeed provide them with meaning, with community, with a contact with the transcendent, and with norms by which to live" (1970*a*, p. 6).

Greeley, like Staude, sees the appeal of occult behavior on the college campus as symptomatic of the alienation of youth from the scientific-rationalist ethos of modern society, with the invocation of new gods: superstition, ecstasy, and "groupism" (Greeley 1970*b*).[3] He notes an affinity between those engaged in occult practices on the campus and the new left, not only in terms of the rejection of the dominant institutional ethos but also in terms of certain value orientations: the affirmation of the inherent goodness of human nature (an echo of both Rousseau and the much older Pelagian heresy) and the concern for transcendent power in human interaction (1970*b*, p. 208). Further characteristics of the new faiths, he suggests, are that they are millennialistic, charismatic, and liturgical. The thrust of Greeley's argument is that the occult revival is to be seen as a neosacral movement in contemporary culture, which by the significance of its presence is further evidence against the proposition that increasing secularization accompanies or is an integral aspect of modernization.[4]

Greeley's writings on the significance of occult behavior among youth, as well as his earlier empirical research (1968), constitute a major critique of the secularization hypothesis.[5] Additional testimony in the occult

[3] A justification for Greeley's terms "tribal Gods" and "tribal consciousness" is the self-designation of some communes as "tribes."

[4] The late Howard Becker had developed, in an unfortunately neglected communication to the American Sociological Association (1960), a theoretical critique of the secularization model. Becker would have no difficulty in perceiving today's occult revival and the neoevangelical movement as instances of "normative reactions to normlessness."

[5] Like Greeley, I see this hypothesis as having been derived from a short-sighted reading of Durkheim and Weber on the modernization process—or else as a correct reading of the mistaken notions of Spencer.

revival literature comes from a recent article of Shepherd (1972), who observes "a new mysticism emerging among the young in the developed countries, one not constrained by an already well-defined religious context" (p. 8). Arguing that the new religious life-style is analogous to the aesthetic experience of music—a theme which echoes the contention of Roszak (1969) that today's dissenting youth have returned to the archaic aesthetic vision of beauty of the shaman, a vision communally shared—Shepherd (1972) proposes that "the counter cultural young among Westerners . . . may be the agents in a process of 'reorientalization' occurring within our own culture, and that they may be the harbinger of spring, of a new value consensus" (p. 8).

In this vein, one might argue that a function of occult practices is to provide a position against what is perceived as "Establishment" mentality with its structural apparatus of modern societies: The oppressive "technocracy," "reductive rationality," and "objective consciousness," to borrow terms from Roszak. Occult practices are appealing, among other reasons, because they are seemingly dramatic opposites of empirical practices of science and of the depersonalization of the industrial order. The appeal of the occult may thus be related to the new appeal of artisan work to many college youth, since it is nonindustrial work, affording a reintegration of personality with the product of one's labor.[6]

One last sociological study of the occult in modern society to be noted here is a recent French collaborative work on contemporary astrology (Defrance et al. 1971). It is not easy to present in a few words the contents of this work which, taken as a whole, is a sociology of astrology, but several of the themes discussed earlier are elaborated here. Edgar Morin (Defrance et al. 1971, pp. 110–25) sees the appeal of astrology among youth today as stemming from the cultural crisis of bourgeois society, with astrology offering symbols of identity as a science of subjectivity. Paradoxically, modern astrology has an antithetical function in modern society: in mass culture, the popularization of astrology in the mass media plays an integrative role in bourgeois civilization by reconciling individuals with their situation. Morin suggests that among counterculture youth astrology is also part of a new gnosis which has a revolutionary conception of a new age, the Age of Aquarius.

In his chapter "Astrology and Society," Fischler (Defrance et al. 1971, pp. 69–81) considers other latent functions of astrology. Modern society multiplies fragmentary contacts between strangers, without traditional norms to guide comportment; in the face of making an increased number of decisions, especially of an interpersonal kind, resort to astrology and

[6] It is tempting to think that both artisan labor and occult work among today's college-trained youth may indicate a new nonindustrial form of inner-directed achievement orientation.

other means of divination reduces the ambiguity of intersubjective conduct. Since astrology is subject oriented, it is a means for self-apprehension and self-grasping. Fischler also presents data initially gathered in 1963 by the French Institute of Public Opinion, which examined in a cross section of the French public just how widely diffused is adherence to astrology. The survey sought the social distribution of those respondents possessing three attributes, namely, they (*a*) knew their own Zodiac sign, (*b*) read their horoscopes fairly often, and (*c*) thought there was some truth to astrological character traits. The results are shown in table 1.

TABLE 1

SOCIAL CHARACTERISTICS OF BELIEVERS IN ASTROLOGY (FRANCE)

Category	Percentage in Total Sample
Total believing in astrology	30
By sex	
Men	21
Women	39
By occupation *	
Professional, managers, and executives	34
Small business owners and artisans ("commerçants")	36
Clerical and sales ("employés")	46
Manual workers ("ouvriers")	29
Farmers and farm workers	15
Not in the labor force or retired	30
By locality	
Under 2,000 inhabitants	21
2,000–5,000	27
5,000–20,000	37
20,000–100,000	34
Over 100,000	40
By age (years)	
20–34	38
35–49	33
50–64	24
65 and over	20

SOURCE.—Defrance et al., *Le retour des astrologues*, p. 74, from survey data gathered in 1963 by the Institut Français d'Opinion Publique, at the request of *France-Soir*.

* Since occupational classification in France differs in some cases from that commonly used in the United States, we have given in parentheses the French category in those instances where the translation is approximate.

Noting the high incidence of astrological adherence among women and those under 35, Fischler suggests (Defrance et al. 1971, pp. 80 f.) that as these formerly segregated strata enter more and more in the modern *polis*, having been less stamped by the belief systems of the dominant culture, they are more prone to recourse to astrology in relating to the larger society.

What is of further interest are additional data presented by Fischler (Defrance et al. 1971, p. 75) indicating the social distribution of two groups: (*a*) those having consulted at least once a card reader, a clairvoyant, or a person predicting the future; and (*b*) those having consulted at least once an astrologer. On the whole ($N = 6{,}000$ in the national sample), the results were consistent with the first survey (the more populated the community, the higher incidence in each group, etc.), save that the category of those 18–25 years of age had a lower incidence (9% and 1.5%, respectively, in the two groups) than the other age categories (which all had practically the same incidence, about 13% and 3%). Although Fischler sees in this that older persons go in for more applied astrology and the young for a more speculative curiosity, it may also be the case that the young are their own practitioners of astrology (and other divinatory practices), for whom going to professional practitioners (part of the occult establishment) would defeat the purpose of a personal quest for meaning and certainty in an ambiguous world.

Although more suggestive than conclusive, the French data point to a redrawing of a certain sociological image of superstition/rationality as distributed in society. For the greatest spread of adherence to astrology is not to be found in the countryside among farmers or among the lower rungs of the occupational structure, but rather in the most densely populated urban centers and among white-collar workers. Moreover, although the French materials are silent on this, my own observations suggest that there is a higher incidence of belief and interest not only in astrology but also in other occult sciences among those of high educational achievement —college and university level—than among those who have not gone beyond high school or those who are still in high school. Obviously, this is a terrain for further empirical studies.

In examining various sociological writings dealing with the occult revival, a number of themes have been touched upon. Most significant, perhaps, is that this phenomenon has to be seen in terms of a broader social context of cultural change. As a spiritual reaction against the rationalistic-industrial-bureaucratic ethos of modern society, it is part of the counterculture. Similarly, it may also be seen as part of a new religious cultural revitalization, having an affinity with both the exuberant neoevangelical movement crossing denominational lines and the equally vigorous political movement of the new left in developed societies.

To take the sociological interpretation of the occult in modern society further along, there is need to develop a more articulated conceptualization. For one thing, we need to differentiate basic elements of the occult, and for another, to reexamine how the occult relates to modernization in its cultural aspect. This will be undertaken in the following section.

COMPONENTS OF ESOTERIC CULTURE

The previously discussed sociological literature on the occult revival has related it, on the one hand, to the sociology of mass society, and on the other, to the sociology of religion. Since we posit that the sociology of the occult is also germane to the study of cultural change and the dynamics of modernization, it may be located more broadly as part of the sociology of culture. It is at this point that I wish to explicate some basic terms of central importance to this essay: "occult," "esoteric," and "secret society." These are familiar terms, yet ones which have had little currency in standard sociological reference works.[7] It is necessary, before conceptualizing these terms, to have a preliminary understanding of "culture." The sense of culture which is followed here is, so to speak, that of a collective paradigm which provides the basic interpretations and justification of ongoing social existence. As the anthropologist Ward Goodenough has expressed it: "Culture then consists of the 'concepts' and 'models' which people have in their minds for organizing and interpreting their experiences." It should be noted that Goodenough takes into account nonlinguistic aspects of culture (which have a bearing on the significance of symbols in esoteric culture, since symbols and imagery are primary modes of relating to reality for the latter) when he adds: "nonlinguistic forms have systematic relationships to each other in paradigms" (quoted in Singer 1968, p. 538).

Equally pertinent here is the conceptualization of Parsons viewing culture as that integral component of systems of social action which provides the fundamental symbolic grounds of expression to the existential problem of meaning inherent in social existence. Although "meaning" has a cognitive and rational orientation, it also has a complex moral one as well, one involved in the evaluation of social action. As Parsons states: "The highest level of the problem of meaning is that of the conceptions of ultimate reality, in the religio-philosophical sense. This concerns the major premises in which the nonempirical components of a culture's total belief system are rooted" (1961, p. 970).

[7] In the sociological literature on the occult revival, I have found commonly known instances of the occult (astrology, witchcraft, etc.) but no analytical definitions or conceptual classificatory scheme subsumed under this rubric. The *International Encyclopedia of the Social Sciences* has no entry for "esoteric" or "occult" and only passing mentions of "secret societies" in a few substantive articles. The UNESCO-sponsored *A Dictionary of the Social Sciences* does have the following comment (by Kenneth Little, an anthropologist) under "Secret Society": "It is difficult [in this context] to discuss the nature of modern institutions, such as Freemasonry, but in primitive cultures secret societies generally constitute an integral part of the social system concerned" (in Gould and Kolb 1964, p. 624). Why the difficulty is not made clear

There is one qualification that needs to be introduced to the above conceptualization of culture, since it has a crucial bearing on the central thesis of this paper. It is that a given social complex, as in the case of modern Western society, may have more than one set of major premises present in its cultural matrix; that is, that there are several cultural paradigms offering the ground of meaning of social action, albeit one set may have dominance in the institutional fabric of society while another set remains covert or latent.[8] The cultural paradigm which is manifest in public institutions, a set of cognitive and evaluative orientations publicly recognized and legitimated in the network of social institutions, is what I propose to call "exoteric culture." Exoteric culture provides the ground of meaning and orientation for the everyday social world. It is the social basis of what the phenomenologist Husserl designated as "the natural attitude," in terms of which actors take the existence of the world for granted, that is, in a sociological sense they take as nonproblematic the institutionalized structures of the social world. It is implicitly in reference to exoteric culture that sociologists and anthropologists, for the most part, have formulated their conceptualizations regarding culture and society.

I wish to propose that a unitary conceptualization of the cultural system of Western civilization in its historical development via modernization has to be modified, and that to arrive at a more sophisticated understanding of cultural systems and societal change there is need to consider what, for heuristic purpose if nothing else, I will call "esoteric culture." It is at this juncture that we need to specify three major components of this esoteric culture, elements common to any cultural whole but taking specific forms in esoteric culture: a set of beliefs and doctrines (cognitive and moral orientations), a set of practices oriented to empirical action, and a social organization within which action is patterned or structured.

Although "esoterism" and "occultism" are often used interchangeably, and although there is no standard agreement as to their referents, it may still be fruitful to venture an analytical differentiation. Both, of course, refer to something which is not immediately given to the senses or to perception, to the nonempirical in this sense.

By "occult" I understand intentional practices, techniques, or procedures which (*a*) draw upon hidden or concealed forces in nature or the cosmos that cannot be measured or recognized by the instruments of modern science, and (*b*) which have as their desired or intended consequences empirical results, such as either obtaining knowledge of the empirical course of events or altering them from what they would have been without this intervention. Obviously, I exclude for the purpose of my analysis some

[8] The conceptualization and research of Florence Kluckhohn (1950, 1961) on dominant and substitute value orientations is highly relevant here.

broader considerations of occult phenomena, such as extrasensory perception and *déjà vu* experiences, because these are harder to integrate within a sociological scheme. To go on further, insofar as the subject of occult activity is not just any actor, but one who has acquired specialized knowledge and skills necessary for the practices in question, and insofar as these skills are learned and transmitted in socially (but not publicly available) organized, routinized, and ritualized fashion, we can speak of these practices as occult sciences or occult arts.[9]

Commonly recognized occult practices include a variety of phenomena, such as those designated as "magic"[10] and divinatory practices,[11] which are very numerous cross-culturally (astrology, the Tarot, and the I Ching having been particularly noted in the contemporary occult revival); they also include practices which are oriented to changing the physical nature of nonhuman objects by the active participation or ego involvement of the subject, as in the case of alchemy.

By "esoteric" I refer to those religiophilosophic belief systems which underlie occult techniques and practices; that is, it refers to the more comprehensive cognitive mappings of nature and the cosmos, the epistemological and ontological reflections of ultimate reality, which mappings constitute a stock of knowledge that provides the ground for occult procedures. By way of analogy, esoteric knowledge is to occult practices as the corpus of theoretical physics is to engineering applications. But a crucial aspect of esoteric knowledge is that it is a secret knowledge of the reality of things, of hidden truths, handed down, frequently orally and not all at once, to a relatively small number of persons who are typically ritually initiated by those already holding this knowledge. Moreover, it should be added, this knowledge is not of a detached or objective sort about an outer reality which stands against the observer as this page stands against the reader; esoteric knowledge is of a participatory sort, namely a knowledge (or gnosis) of the meaning of the world to human existence, in the progressive realization of which the subject develops internally and liberates himself from the strictures of everyday life.

Since esoteric knowledge is taken as knowledge of the real but concealed nature of things, of ultimate reality (a knowledge which therefore is divine, theosophic in the generic sense), it is important that its recipient

[9] It is worth noting that occultism has both a scientific and an aesthetic aspect, which relates back to our earlier remarks on the meaning of the occult revival for youth. We need not dwell on how much of scientific creativity has a pronounced aesthetic dimension, and conversely

[10] Parlor tricks or stage magic is not meant here, but rather such practices as sorcery, witchcraft, ritual or ceremonial magic, and the like

[11] For a succinct anthropological statement on divination, see Victor Turner (Sills 1968, 13· 440)

be demonstrated worthy of receiving it; that is he must meet criteria of acceptance into the inner circle of true knowers. Hence the need of the candidate to submit at various stages to a series of trials and ordeals, in the course of which the adept becomes increasingly socialized into the esoteric culture and increasingly desocialized from the natural attitude of the exoteric culture.[12]

At the heart of esoteric knowledge is its concealment from public dissemination, from the gaze of the profane or uninitiated. To shield it from vulgarization, it is presented to the adept not directly but, typically, symbolically or metaphorically, so that it has to be deciphered progressively by the neophyte who uncovers layers of meaning in stages of initiation. As a correlate of this, the social organization and handling of esoteric culture tends to take the form of secret societies, societies whose modes and codes of organization and membership lists are not publicly disclosed and which may even be incompletely known to members who have not attained the highest stages of initiation and spiritual perfection.

The sociology of secrecy and secret societies, which is essential for any consideration of esoteric culture, has largely lain fallow since the seminal essay by Georg Simmel (1905). Yet secrecy is of general sociological interest since, as Simmel was quick to note, it is constitutive of social structure and social interaction. Even the most democratic countries, de jure or de facto, organize much of their affairs secretly and have agencies which specialize in concealed activities; all information about the doings of formal organization is not for public consumption but only to qualified insiders, some more than others. This leads to other outsiders seeking to obtain this information in specialized ways (private and governmental espionage activity seeking industrial, military, and state secrets), with such information-gathering activities being themselves secret.

To be sure, not all secret societies pertain to esoteric culture, nor are all social organizations that have secrets, secret societies. But those secret societies which are social forms of esoteric culture have common features. They typically have rituals of initiation and are hierarchically structured in terms of strata which correspond to different degrees of initiation. Leadership and authority are functions of stages of acquisition of esoteric knowledge, at least in theory. At the top echelon is a very small elite designated variously as "Magi," "Grand Masters," or other appropriate terms, and a council responsible for ultimate internal and external policies of the

[12] The academic world may be seen, through one perspective, as routinizing and secularizing the esoteric acquisition of gnosis At lower levels, students progress through neophyte levels known as grades. At higher levels we have *rites de passage* (qualifiers, Ph D. orals, and the like) as we initiate students into higher stages of the academic fraternity. The Phi Beta Kappa ceremony is a symbolic accolade of those who have shown particular aptitude for academic mysteries, albeit few of its recipients may be aware of its esoteric background.

organization. The council is somewhat along the lines of an executive committee of formal organizations; an enlarged board of directors may include, as notably in the case of Freemasonry, persons holding high ranks in exoteric society, even including heads of state.[13] Although the hierarchic principle of organization is basic to secret societies, the higher rungs are accessible to all. Moreover, fraternal solidarity is greatly emphasized: irrespective of social standing in the larger society, all members are brothers (or sisters), and when in a situation of distress, a member is to be given every assistance possible by any other member at hand, even on the battlefield, if they belong to opposite forces.

These, then, are major components of esoteric culture. It would go beyond this essay to articulate specific esoteric belief systems or to discuss the details of organization of specific secret societies. More relevant is to indicate that esoteric culture is not concretely disjoint from exoteric culture, that it coexists, albeit unobtrusively with the latter,[14] or stated differently, that there are many interchanges between them.

The esoteric culture often uses as referents publicly known cultural materials such as religious texts and figures (e.g., the Torah, the Book of Revelations, Adam, Christ, etc.), but it considers that the meaning of these is not exhausted by their public definition and recognition; rather, the esoteric group sees itself as the true repository of the ultimate reality manifest in the materials in question. Only a select few, initiated into the mysteries can decipher and pass on in an unbroken chain the real meaning of what underlies the figurative. Only a few, thus, can be permitted to attain knowledge of the secret name of God which Moses learned on Mount Sinai and which has been transmitted orally to those who can correctly understand the Kabbalah; only a few can learn the secret teachings of Christ to His chosen disciples, etc.

Since these secrets are those that reveal the ultimate nature of reality, the concealed forces of the cosmic order, it means that esoteric knowledge is an ultimate source of power, which must be shared and utilized by a relatively small group of initiates. Such power is never justifiable in terms of the enhancement of the material conditions of the esoteric knowers but rather in terms of broad impersonal ends, humanitarian ideals, etc. Consequently, much of the parlance of esoteric culture is necessarily

[13] This is institutionalized in Sweden and Great Britain where the monarch or a member of the ruling family, respectively, is the titular head of Masonry in his country; though not institutionalized in the United States, most presidents have been 33° Masons or of high rank.

[14] The cultural traditions of all the major complexes of civilization and high religions have an esoteric side. Thus, Islam, Christianity, Hinduism, and Judaism have their esoteric components in Sulfism, theosophy (in various forms: Illuminism, Rose Croix, and more modern versions such as those of Blavatsky, Gurdjieff, R. Steiner), Tantric Yoga, the Kabbalah, etc.

obscure, that is, designed to put off members of the larger society; this is in some ways similar to the argot of the underworld or even to the language of some psychotics used as a shield from public deciphering. Exoteric language glosses over the esoteric source of such expressions as "third degree" (from Masonic initiation), "magnum opus" (from alchemy), or "sub rosa" (from Rose Croix), just as other items originating in the esoteric culture have been absorbed, for example, card games (the major suits trace back to the Tarot, the esoteric depiction of human existence seen as a dialectical process).

ESOTERIC CULTURE AND SOCIOCULTURAL CHANGE

It is the relation of esoteric culture to various facets of Western modernization that is of particular interest to this article. We wish to argue that much of what is modern, even the ideology of modernization at its source, has originated in esoteric culture; paradoxically, the value orientation of Western exoteric society, embodied in rationalism, the scientific ethos, and industrialism, has forced esoteric culture into the role of a marginal or underground movement. That is, modern Western civilization (dating it back to the Renaissance and Reformation) has increasingly given to esoteric culture the mantle of a counterculture, while at the same time coopting many of its values and products.

I would like to suggest that the conceptions of ultimate reality in the esoteric tradition may be conceptualized as part of the latency subsystem of Western society, following Parsons's model of structural differentiation of social systems of action (Parsons 1969). Further, we contend that in the Western case, at least, (*a*) such esoteric conceptions and modes of interpreting reality form a cultural paradigm which provides leverage against the institutionalized paradigm, hence function as a seat of inspiration for new systems of social action; and (*b*) that at various historical points these conceptions and modes have come into play in the larger society so as to provide vehicles of social and cultural change.

Moreover, albeit to document this point would require much more space than here available, the basic cognitive model of hidden, underlying reality central to esoteric thought, is that of a reality moved by forces, by energies constantly in motion and in tension with one another; it is a model opposed to the static, stable, or harmonious view of things inherent in the natural attitude. Hence, we suggest, at the very heart of the ideology of modernization, or modernism, is an esoteric influence. This ideology, which positively evaluates what is new as against what is old, which sees the unfolding present as a time of liberation and freedom from the yoke of the stagnating past, is an ideology of a new order of things to come and of a this-worldly salvation by human means—an ideology which at least one

author (Voegelin 1952, p. 133) has seen as a product of esoteric (gnostic in this instance) symbolism manifested in the development of the Puritan Revolution.

Of relevance here is to discuss the relation between esoteric culture and avant-garde culture, for the esoteric apprehensions, depictions, and interpretations of reality find a ready-made terrain of expression in artistic products (literature, painting, architecture, even music) whose esoteric significance escapes the larger public. To the initiates such symbols are more than artistic: they also convey a message, and hence are expressive symbols in the fullest sense.

In a recent article pointing out the antithetical relation of the social structures of industrial-technological society and modern culture, Daniel Bell (1970) has noted in the latter the prevalence of a dominant impulse "towards the new and the original . . . so that the *idea* of change and novelty overshadows the dimensions of actual change" (1970, p. 17). This ideology of change, of modernism per se, is one which Bell locates in the cultural tradition of avant-garde art that appeared in the 19th century as a counterculture to the rising bourgeois culture of industrial society.[15]

Bell does not examine any linkage of occultism and the formation of avant-garde in the 19th century, although he proposes in passing (1970, p. 34) that what avant-garde values stand for—antistructure, antihistory, radical freedom, in brief, values of nihilism and anarchism—are part of an older Western tradition, that of gnostic esotericism.[16]

In a complementary article, Vytautas Kavolis has explored recently (1970) the sociopsychological nexus between avant-garde culture and what he calls "Satanic" and "Promethean" personality modes, the former characterized by a "resentment-destruction mechanism," the latter by a "sympathetic concern for the needs of others."[17] These personality orientations are manifested in the activist and nihilistic aspects of avant-garde culture, oriented to both the destruction of the established order of things and to a perpetual innovation and renovation of forms. Moreover, Kavolis sees avant-garde culture as giving positive value to the symbols of the

[15] For a fuller discussion of the rise and characteristics of avant-garde literary culture, see Irving Howe's essay, "The Idea of the Modern" (Howe 1967, pp. 11–40).

[16] In a similar vein, Edgar Morin has spoken of the revival of astrology today as a manifestation of a new gnosis (Defrance et al. 1971, p 123). See also the remarks of Hans Jonas (1963) on gnostic features of some modern political tendencies, including the theme of alienation.

[17] Students of astrological traits might note with amusement the congruence of the Promethean and the satanic with the two sides of the Aquarian-born (today's *enfants du siècle*): "A negative Aquarian is one who . . demands licence under the guise of liberty, and shouts for public service while serving none but himself The positive Aquarian is . . . the humanitarian, seeking liberty not for self, but for others, dealing in the larger affairs of the country or planet, ever urging humanity upward and onward, and running ahead to show the way" (Keane 1967, p. 34).

satanic psychological mode of orientation, symbols treated as the epitome of antisocial values in the established order of society: "To some extent, the avant-garde culture could be interpreted as an attempt to legitimate much of what used to be illegitimized in the Satanic mythology" (1970, p. 27).

The influence of occult themes, particularly those dealing with the demonic and the satanic, is a striking aspect in the historical development of modern avant-garde culture, which is viewed here as both a literary and a political protest against the institutionalization of a rationalistic-industrial bourgeois social order. This protest against modernization was a major common denominator in the Romantic movement, and esoteric culture provided much of the materials for the protest against the new social order, though neither esoteric culture nor avant-garde culture sought a return to the *ancien régime*. Ritual magic, "forces of darkness," Satan himself, became symbols of identification, rallying points, in brief, revolutionary forces drawn from the counterculture of the occult,[18] in the fight against bourgeois values. The occult as a source of inspiration abounds, then, in writings of well-known and lesser-known Romantics, such as Goethe (Lepinte 1957), Novalis, Gautier, Nerval, Byron, Lautréamont, and Baudelaire (Bays 1964).[19]

Such themes of the occult were continued by later generations of the avant-garde culture,[20] notably the "accursed" symbolist poets such as Rimbaud and Verlaine (Senior 1959), but also Yeats and Thomas Mann, and finally, André Breton, the crucial figure of surrealism, a movement of particular sociological significance since it represents a clear articulation of artistic protest and political radicalism.[21] Many of the surrealists (Breton himself, Aragon, Eluard, Naville) were or are politically committed to the radical left, and the influence of surrealism is even to be

18 For a fuller statement on the social significance of Satan in the last century, see the cogent remarks of Eugen Weber (1964)

19 Note here the observation of Howe (1967) in commenting upon a passage of Baudelaire "[This seems] the report of a desire to create . . . the very ground of being, through a permanent revolution of sensibility and style, by means of which art could raise itself to the level of white or (more likely) black magic" (p 17).

20 To do full justice to the role of esoteric culture as an inspirational source for avant-garde culture, one should also examine esoteric influences on earlier literary and artistic innovators, such as Shakespeare (Arnold 1965; Reed 1965) and Rabelais (Masters 1969), to say nothing about esoteric currents in painting and architecture (Van Lennep 1966)

21 In commenting on "Surrealism and Revolution," Camus stated: "The essential enemy of Surrealism is rationalism. Breton's method . . presents the peculiar spectacle of a form of Occidental thought in which the principle of analogy is continually favored to the detriment of the principles of identity and contradiction. . . . Magic rites, . . . alchemy . . . are so many miraculous stages on the way to unity and the philosopher's stone" (Howe 1967, p 218)

found in the writings of A. Césaire, one of the founders of *Négritude*, the cultural arm of black liberation. Breton drew upon various sources of inspiration (including Marx, Freud, and occultism) to formulate a revolutionary consciousness aimed against the bourgeois world (of utility, reason, realism, and technological society), a consciousness whose intention "is always to allow the *irruption* of 'wild' images that will disturb the sensibility by shattering the coherence of those 'stable' images that make up, for each individual, the objective world (Willener 1970, p. 224).

The contemporary significance of surrealism as a source of inspiration for the May 1968 revolutionary movement in France has been fully discussed in an excellent sociological study by Willener (1970).[22]

In the political development of modern society, both in the West and in the Westernization (including imperial domination) of the "Third World," esoteric culture has also had an influence in avant-garde political movements and ideologies which have pitted themselves against established regimes. A major social vehicle of such protest have been secret societies: Weishaupt's Bavarian Illuminati in the 18th century, Freemasonry in France in the 18th and 19th centuries, the Carbonari in Italy and France, Mazzini's Young Italy, the Sinn Fein in Ireland, and many others. For the most part, the ideology of such secret societies was nationalistic, republican, anticlerical, even internationalistic. All these drew upon the imagery and expressive symbols of esoteric conceptions of reality, especially symbols of the liberation of man from the yoke of darkness (politically interpreted as the yokes of traditional political institutions or alien oppressors) into the realm of light, of freed humanity. For the most part, these movements succeeded in establishing political regimes which at least partly satisfied their aspirations.[23]

Assuredly, not all secret societies drawing upon esoteric symbols, rituals, and interpretations can be classified as progressive, for there is the minority case of those having a reactionary image, such as the Ku Klux Klan in the United States, the Cagoule in the France of the 1930s, and even, in the light of the discussion provided by Pauwels and Bergier (1968), there were esoteric influences operative in secret societies (such as the Thulé order)

[22] The hexing of the Pentagon and the Stock Exchange (to say nothing of an Establishment department of sociology) and the adoption of the acronym WITCH attest in their own modest way to the appeal of occult symbolism in some sectors of radical students. To my knowledge there was no manifest influence of surrealism in these instances.

[23] Thus, the very symbols of the Great Seal of the United States (the luminous delta, the pyramid) are esoteric symbols of Freemasonry, to which belonged most of the founding fathers; similarly for the symbols of the French Republic (especially that of 1848), whose motto "Liberty, Equality, Fraternity" represents cherished ideals of Masonry formulated in lodges before the French Revolution. Simón Bolívar, the liberator of South America from Spanish rule, was also imbued with Masonic ideals, as was Garibaldi in Italy, etc.

that played a covert role in the formation of nazism. This is perhaps indicative of the fact that esoteric culture, with its fantastic wealth of imagery and symbolism, is multivalent in terms of political expressions that can be derived from it. Yet, whatever the specific instance, it may be said that esoteric culture provides leverage against the existing order by grounding political reflection and action in a reality that transcends that of everyday life,[24] but which is a reality that may become actualized in the historical future by reversing the present order of the world.

One other aspect of the political expressions of modern counterculture to be noted is that of Marxism. Its crucial leverage against bourgeois mentality is its formulation of dialectical materialism, which provided Marx and Engels the key with which to unlock the hidden laws of the historic process. The intellectual progenitor of modern dialectics is commonly taken to be Hegel, but the recent study of Benz (1968) has demonstrated the extent to which esoteric and mystical sources figure in the German intellectual background of Hegelian and Marxist thought. Notable sources are theosophy, philosophical alchemy, and the Kabbalah, drawing much inspiration from the writings of earlier mystics such as Meister Eckhart and particularly Jacob Boehme, and reinterpreted in early 19th-century German theosophic and evangelical circles, as well as philosophical circles which also partook of the Romantic movement.[25]

A last consideration involving the outputs of esoteric culture to the development of modern Western civilization is its relation to scientific thought. The latter in its empiricist and positivistic image of an objective reality, measurable by empirical means and existing independently of the human subject, is perhaps the key mode of thought in the modernization process which has illegitimated and devalued the practices of occult science. Yet, paradoxically, esoteric influences in the form of symbolisms, imagery, practices, and cosmologies, have been in much of the background of the rise of scientific disciplines.

In the case of modern depth psychology, Bakan (1958) has brought out

[24] S N Eisenstadt's evaluation of the ability of Chinese Communists to draw upon various threads of protest in Chinese society and wield them into a common cause is highly germane here. He signals in particular that such a linkage "enabled some gentry groups . some secret societies, some warlords and some peasant rebels to go beyond their own restricted social orientation and to find a wider basis and forge out new, broader orientations" (1971, pp. 49 f.). It is our contention that these orientations are likely to have originated, at least in part, in esoteric doctrines formulated in secret societies. In the case of China, of noteworthy mention in the rise of national consciousness is the case of the Taiping (Shih 1967).

[25] Among other things contained in the esoteric conceptions of the theologian Oetinger, a generation before Marx, are such themes as an eschatological view of history and the coming freedom of man in a "Golden Age" in which the state, private property, and a money economy will disappear in communal equality and love (Benz 1968, pp. 32–53).

various elements of the Jewish mystical tradition which frame much of Freud's psychoanalysis; the latter's theory of the libido and the significance of symbols in the psychic process (including the analogic linkage of symbols) may be seen as a scientific formulation of elements of the Kabbalah and the Zohar. His one-time heir apparent, Jung, made intensive studies of medieval and Renaissance alchemy (Jung 1968*a*, 1968*b*) which he linked to personality development. Freud was early influenced by Charcot's experiments in hypnotism, but the foundations of hypnotism had been laid nearly a century before by the occultist Mesmer's demonstrations of magnetism (Mesmer 1971).

Even in the natural sciences esoteric influences have played a not insignificant role, which can only be mentioned in passing. If today the alchemical symbols of chemical elements remain a glossed-over vestige, the development of chemistry from its matrix in late medieval alchemy deserves mention (e.g., Stillman 1960). Alchemy and astrology may also have been of importance in the rise of modern medicine, with Paracelsus being another key figure mediating between esoteric culture and modern scientific thought—the innovation of operating on human bodies may have been guided by an attempt to establish the correctness of astrological views that different parts of the body, and consequent pathologies, are under the influence of different signs of the Zodiac. Astrology and theosophy were also part of the cultural baggage which was utilized rather than rejected by modernizing scientists, such as Kepler and even Newton (Hutin 1960). The very social organization of modern science, in the form of academies of science, owes much inspiration to Francis Bacon's *New Atlantis* with its "scientists' Paradise," a work said to come out of the "Hermetic-Cabalist" stream (Yates 1964, p. 450).[26]

So much for some indications, necessarily brief and incomplete, of the range of esoteric influences in the historical process of modernization.

CONCLUSION

Marginal as the occult revival may initially appear to sociological preoccupations, the study of the esoteric in fact touches on many facets of our discipline, such as the sociology of knowledge, the sociology of art, the sociology of religion, and the sociology of deviance.[27] For heuristic

[26] Once organized, the scientific community broke formally with esoteric culture This was symbolized in 1666 upon the establishment of the French Academy of Sciences, membership in which excluded the practice of astrology. A few years before France had still had an official state astrologer, Morin de Villefranche, professor of mathematics at the prestigious College de France, who had sought to modernize and rationalize astrology with mathematical precision.

[27] In the context of the latter, see the study of differential reactions to witchcraft

purpose, this essay has mainly placed the emphasis on esoteric culture as an important promulgator of a counterculture of long standing in the West. Essentially, we have viewed the function of esoteric culture to be that of a seed-bed cultural source of change and wide-ranging innovations in art, politics, and even science, analogous to the functions of "seed-bed societies" discussed by Parsons (1966) in the case of Israel and Greece.[28]

In discussions of social change elsewhere (Tiryakian 1967, 1970), I have proposed that important ideational components of change (i.e., changes in the social consciousness of reality) may often originate in non-institutionalized groups or sectors of society whose paradigms of reality may, in certain historical moments, become those which replace institutionalized paradigms and become in turn new social blueprints. Relating this to the present essay, I would propose that esoteric culture, and groups of actors mediating esoteric to exoteric culture, are major inspirational sources of cultural and social innovations.

To document and validate this model is obviously no easy matter. It involves demonstrating the meaningful sociohistorical affinity between seemingly heterogenous social spheres of action, a methodological problem of exactly the same nature as the one involved in Max Weber's study of the affinity of ascetic Protestantism and the ethical basis of modern capitalism. The magnitude of the problem involved in relating esoteric culture to social innovations in exoteric culture is even greater than the Protestant case. It involves developing tools of analysis which will enable us, as sociologists, to make sense out of esoteric texts and documents, many of which depend on being able to decipher the meanings of expressive symbols which are by their nature qualitative expressions of reality, and not subject to quantitative measurement in their presentation. A promising avenue here may be the techniques of linguistic and structural anthropology, for example those used in deciphering mythologies, which have similarities to esoteric conceptions of reality. The increasing applications of phenomenology (which is, after all, oriented to the inner grasping of structures of consciousness) in the social sciences (Natanson 1972), such as those being developed by ethnomethodology, may be particularly fruitful in this vein.[29]

The problem of indicating linkages between esoteric symbolisms, imageries, and conceptions of reality to purposive social behavior, that is, the

in England and on the Continent by Currie (1968). For other materials on witchcraft, deviance, and social structure, see the New England study of Erikson (1966)

[28] We might note in passing that Western esoteric culture has deep historical roots in cultural aspects of both Israel and Greece, notably the prophetic and Kabbalistic traditions of the former and the rituals of the mystery cults of the latter

[29] A student of Harold Garfinkel, Trent Eglin, is presently preparing an ethnomethodological study of alchemy.

question of how conceptions of the structure of reality translate into paradigms of social action and social imagery (e.g., how the esoteric notion of the androgyne, Adam Kadmon, is linked today to fashion designers of unisex clothes) is also laden with serious methodological difficulties. And this is rendered even more serious by the fact that major mediating groups between the two cultures tend to operate in socially invisible or secret social organizations, which means a paucity of readily available informative documents, or in some cases, an undue reliance on records of governmental and other institutional agencies which have sought to repress these esoteric groups.

Nonetheless, these methodological vexations should be seen as more of a challenge to the sociological imagination than in principle insoluble. The very framing of theoretical questions concerning the relation of esoteric to exoteric culture, particularly in the area of conceptualizing the dynamics of social change, may suggest new methodological developments to the sociological enterprise.

A final note on the occult revival may be apposite in reconsidering the crucial notion of the modernization process. It may be fruitful to view modernization in a broader historical context than just that of the past 200 years or so, to view it instead as stretching back to antiquity, in the course of which modern ideas have dislodged previously institutionalized paradigms during "crucial periods"[30] of social change—a recurrent feature in the development of Western society. The net effect has been, to venture an analogy, a stochastic process of change rather than one of continuous development, one punctuated by adaptive mutations in the cultural code of Western civilization, to propose yet another analogy.

In the historical unfolding of Western civilization, occult revivals have attended such crucial periods of transition from one cultural matrix to another. The waning period of the Roman Empire is a case at hand, with a great flourish of esoteric culture and symbols (much of which was absorbed in primitive Christianity prior to its institutionalization under Constantine). The Renaissance/Reformation period is another major one of shifting cultural paradigms, representing a rejection of the rationalism of medieval scholasticism and of established ecclesiastical authority, having consequences in a variety of social domains. It is during this period, and not the antecedent medieval period, that there was a major occult revival, with esoteric culture becoming a major vehicle of new expressive symbols and belief systems, a source of new value orientations.[31]

[30] I have taken this suggestive term from Balandier (1971*a*, p 202) in preference to the overworked "periods of crisis." In this and other writings (1970, 1971*b*), Balandier has been developing a theory of modernity which seems particularly fruitful for reformulating the dynamics of change.

[31] For an outstanding historical study of this cultural context, see Yates (1964).

In both of these instances the particular thrust of efficacy of esoteric culture lay, I would suggest, in the exoteric culture having what may be characterized as a loss of confidence in established symbols and cognitive models of reality, in the exhaustion of institutionalized collective symbols of identity, so to speak. There was what may be called a "retreat from reason into the occult" (Yates 1964, p. 449), a retreat not in the sense of a total "leaving of the field," to borrow from Kurt Lewin, but rather in the sense of a religious retreat, a temporary withdrawal for inspirational meditation which provides a restoring of psychic energy to be used in re-entering the everyday life with greater vigor.

The occult revival of today is, in this perspective, comparable with previous such phenomena, even including the contemporary attacks against institutionalized rationality, attacks which take irrational forms in the now generation. To make sense of the irrational, the modes and conditions in which it occurs, as well as possible societal consequences deriving from it, is a basic concern of the sociology of the occult. If we come to perceive the occult revival of today not as an ephemeral fad of mass society but as an integral component in the formation of a new cultural matrix, more likely international than national in scope, if we see it, in brief, as an important vehicle in the restructuring of collective representations of social reality, we will see (with or without the third eye) the Age of Aquarius as a major sociological happening.

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Two Sources of Antiwar Sentiment in America[1]

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Opposition to the Vietnam war has been manifested both in university protest actions and in cross-section public opinion surveys. But the college-related protests and the wider public disenchantment have sharply different characteristics: they have peaked at different points in the war; they are discontinuous in educational and age basis; and a substantial part of the antiwar public is also extremely hostile toward college protesters. Together these findings suggest a distinction between moral criticisms of the goals and nature of the war and pragmatic disillusionment over failure to win it. This hypothesized distinction is investigated using thematic analysis of open-ended responses from a cross-section sample of 1,263 Detroit adults who had indicated opposition to American intervention in Vietnam. A small classroom sample of University of Michigan students is also used for comparison purposes. The themes emphasized by the Detroit sample as a whole, and by most subcategories defined in terms of race, sex, age, and education, are generally consistent with the moral-pragmatic distinction. Other related factors (such as traditional isolationism) are also shown to contribute to broader public disenchantment with the war. The moral-pragmatic distinction, while somewhat oversimplified, is useful in considering public reactions to future wars of the same general type.

Two distinct measures of opposition to American involvement in Vietnam can be traced over the past seven years. One is the intensity and scope of college-related protests against the war. The other comprises the results of Gallup polls and similar opinion surveys based on cross sections of the entire American adult public. Both measures show increasing disenchantment with the war, and it is easy to treat them as simply two aspects of the same thing. There is some truth to this, but even more error. The college-based protest has focused on moral objections to the use of American military power in Vietnam. The general public disenchantment, however, seems to have been largely practical, springing from the failure of our substantial military investments to yield victory. Confusing these two sources of opposition to the war leads to serious

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miscalculations about the relationship between mass opinion and presidential action in matters of war and peace.

THREE DIFFERENCES BASED ON PAST SURVEY FINDINGS

The first major campus protest occurred in March 1965 at the University of Michigan. A handful of faculty members and students created the "teach-in" as a way of arousing opposition to the recently initiated bombing of North Vietnam. Several thousand members of the university community attended the all-night series of lectures and discussions about the war, concluding on an emotional note with songs and oratory. Above all, there was a strong tone of moral indigation in the teach-in. The United States was pictured as intervening in what was essentially a civil war, supporting a corrupt and unpopular government, and now extending the war dangerously by sending modern bombers to pound the countryside of North Vietnam. The main issue presented was not whether the United States could win the war but rather the devastation that such a victory would entail.

Similar protests spread rapidly in the next months to other major campuses. Later, they expanded beyond the campus and led to massive demonstrations in Washington, New York, and other cities. This expansion was characterized by the same moral emphasis seen in the first teach-in. A whole generation of students, especially at major universities, is learning to criticize our involvement in Vietnam as not merely unsuccessful but also morally wrong.

It is commonly assumed that the public slept until awakened by the college protests. Since then, public opinion is thought to have moved in much the same direction, though more slowly, and with uncertainty and occasional backtracking. This movement of national opinion can be traced by the one question that the Gallup organization has repeated regularly since 1965:[2] "In view of the developments since we entered the fighting, do you think the United States made a mistake in sending troops to fight in Vietnam?" Results for this question show a large and unmistakable trend in growth of opposition to the war over the seven-year period. In August 1965, only 24% of the population believed our intervention a mistake; by May 1971 that figure had climbed well past a majority to 61%.

So far it appears that public opinion has followed much the same course as the college protest. But a closer look at dates and events reveals some

[2] These and other Gallup findings reported came from the monthly publication *Gallup Opinion Index*. Further descriptive analysis of these results and of related data from the University of Michigan Survey Research Center and other sources can be found in Converse and Schuman (1970), Davis (1970), and Mueller (1971).

important differences. The first teach-in, for example, was created to protest a major new employment of military force by the United States—the bombing of North Vietnam. The countrywide campus strikes in May 1970 were directed in good part against still another example of expanding American military power—the thrust into Cambodia. These two examples reflect the fact that university protests have been provoked primarily by anger and dismay over *offensive* military actions by the United States.

When we search the polls for similar turning points in the trend of general public opinion, we find them at different locations. The most dramatic change in survey trends on the war is best reflected by a "hawk-dove" question that Gallup administered at several points in 1968 and 1969: "People are called 'hawks' if they want to step up our military effort in Vietnam. They are called 'doves' if they want to reduce our military effort in Vietnam. How would you describe yourself—as a hawk or a dove?" Just before the Tet attacks in January 1968, with American leaders confidently predicting victory, the number of self-described hawks outnumbered doves by two to one. But two months after Tet the proportion of doves in the country slightly exceeded that of hawks, and by the end of the same year, doves outnumbered hawks by nearly two to one. The shift in a space of just 60 days represents probably the largest and most important change in public opinion during the entire war.[3] (The "mistake" question quoted above also reveals a sharp drop in support for the war over the first eight months of 1968; the slope is less precipitous probably because the question was less fitted to immediate policy directions.) These transformations came, it will be noted, not in reaction to expanding American power, but in response to a widely advertised American defeat.[4] It is almost as though a sizeable number of Americans had

[3] The figures for several key dates are.

	Hawks	Doves	No Opinion	Total
(*a*) January 1968	56	28	16	100
(*b*) March 1968	41	42	17	100
(*c*) April 1968	41	41	18	100
(*d*) November 1969	31	55	14	100

The Tet Offensive occurred between the (*a*) and (*b*) measurement points, while President Johnson's announcement of a partial bombing halt (and his decision not to seek reelection) occurred between (*b*) and (*c*). Thus the decisive change in early 1968 seems attributable to the offensive itself, rather than to the presidential policy announcements The subsequent decline in number of hawks over the next year and a half is no doubt more complicated and represents the basic acceptance by both political parties of the bombing halt, the Paris talks, and later the troop withdrawals. By the end of 1968, almost all political leaders had become "doves," at least in rhetorical expression.

[4] Oberdorfer (1971) offers a persuasive case for the official American interpretation of Tet as a military defeat for the Viet Cong and North Vietnamese. But he also

suddenly concluded that the war was not about to be won by being "stepped up," and that hence the only alternative was to step it down. More generally, the curve of broad public disenchantment with the war seems to reflect not offensive actions but news of defeats, casualties, and frustrations.[5]

A second divergence between campus-related protests and general public opinion appears in the reaction of the public to antiwar demonstrators. The gathering of more than a quarter million people in Washington in November 1969 drew heavily from colleges and universities but was intended as evidence of the extent of opposition across the country to the administration's prolongation of the war. Great pains were taken to keep the demonstration peaceful, so that it would appeal to the public at large. Yet the following month the Gallup Poll showed a 6% *rise* in public approval "of the way President Nixon is handling the situation in Vietnam." The president's speech a few days before the march undoubtedly had some influence in rallying public opinion to his side. But there is also reason to think that the demonstration itself had a negative effect on parts of the public unhappy with the war but even unhappier with demonstrators.

Poll data show clearly that open protest against the war is not well regarded by the great majority of American adults In 1968 the University of Michigan Survey Research Center asked a national sample to indicate their "feelings" toward "Vietnam war protesters" on a scale ranging from zero (very unfavorable) to 100 (very favorable), with a neutral midpoint of 50. It is perhaps no great surprise that seven out of every 10 adults placed protestors on the negative half of the scale. What may seem strange is that extreme dislike of war protestors was shown by many people who on other questions indicated their own opposition to the war. For example, one question in the survey asked "Which of the following do you think we should do now in Vietnam?" Three alternatives were given:

1 Pull out of Vietnam entirely.
2 Keep our soldiers in Vietnam but try to end the fighting.
3 Take a stronger stand even if it means invading North Vietnam.

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documents graphically the way in which the Communist offensive was interpreted by the American public as new and dramatic evidence that the war was far from over and far from being won.

5 Mueller (1971) suggests that both the Korean and the Vietnam wars can best be explained in terms of an initial "rally round the flag" enthusiasm, followed by a drop in support as the costs, frustrations, and length of each war became clear. The Chinese intervention in Korea had an impact even greater than Tet in shattering expectations of a quick and decisive victory Mueller seems to argue against the importance of any single similar traumatic event for the Vietnam war, but he does not deal explicitly with Tet, or with the hawk-dove question we have reviewed here

Of those who chose the first response, calling for unilateral withdrawal, more than half nevertheless placed "Vietnam protestors" on the negative side of the feeling scale. One out of four of these extreme doves placed protestors at the absolute bottom of this 100-point scale. These findings raise serious questions about the effect of massive antiwar demonstrations. When the television cameras focus on the protestors themselves, rather than on the object of protest, Vietnam, the demonstrations probably lead many people who are against the war toward support for the president.

One more important set of findings from national surveys points to the divergent types of antiwar sentiment. As we have already noted, the most forceful dissent over Vietnam has come from students and faculty in leading universities. These articulate opponents of the war have tended to assume that their potential allies in the general public are the most educated and informed segments of the population. Such is indeed the case on many issues—for example, questions involving civil liberties—where the universities provide forward positions which then find public support in direct proportion to the education of those questioned (Stouffer 1955).

Contrary to common belief, however, this has not been the case with the Vietnam war. Analysis of poll data shows more educated sections of the public to have generally provided the greatest support for continuing American involvement. In February 1970, for example, Gallup asked its national sample: "Some U. S. Senators are saying that we should withdraw all our troops from Vietnam immediately—would you favor or oppose this?" Of those having an opinion, more than half the grade-school-educated adults favored immediate withdrawal, about two-fifths of those with high school backgrounds, and only 30% of those with at least some college. This was not a fluke. In May 1971, 66% of those college-educated persons with opinions claimed that the war was a mistake, but the figure rose to 75% among the grade-school-educated. In general, a careful review of public opinion data over the last seven years shows that on most war-related issues, the greatest opposition to continued American involvement in Vietnam has come from the least educated parts of the population.[6]

A related finding is that when it comes to Vietnam, the "generation gap," at least in a simple form, has been largely a myth. Age differences in the general public have been neither great nor consistent. On the

[6] The socioeconomic findings from survey data are supported by the results of census tract analysis using cities and towns that have carried out referenda on the Vietnam war (Hahn 1970). More detailed analysis *within* the college category reveals that opposition has been great in high-quality college groups, exactly as one would expect from the fact that the protest movement began at such places as Columbia, Harvard, and Michigan (see Converse and Schuman 1970; Robinson and Jacobson 1969).

question about immediate withdrawal mentioned above, those under 30, those 30–49, and those over 50 all show much the same pattern of responses. More recently, young people do call for a faster rate of withdrawal, but older people continue to be more likely to regard the war as a mistake.

What sense can we make out of these poll results, especially when they contradict what our eyes and ears reveal about the intensity of antiwar feeling among youth on college campuses? The first thing to realize is that college students comprise less than half the college-age population in the United States. More particularly, it would be quite possible to have every student in the major universities in complete opposition to the war, yet find the total college-*age* population showing strong support. To this we must add the obvious but easily forgotten fact that in national surveys, "college"-educated persons are primarily adults who are well past college age at present, so that we cannot expect them to reflect recent changes on campuses.

Once we realize that students (and faculty) at Columbia, Michigan, or Berkeley cannot tell us much about the degree of public opposition to the war, we must also recognize a more subtle point: the *basis* for disenchantment with Vietnam need not be the same in the general population as on the campus. Why, indeed, is there public opposition to the war? This is such a simple-minded question that it may seem absurd even to raise it. The fact is, however, that we do not know the answer. Gallup and other polls have documented well the growing negative sentiments on the war, but almost no effort has been made to explore the reasons behind these sentiments. Such exploration requires open-ended questions that ask people to state in their own words why they hold a particular policy position.[7] Questions of this sort, however, are expensive to include in interviews and complex to analyze and report. Unfortunately, their omission has a more dangerous effect than simply leaving us ignorant, for in the absence of knowledge of public opinion we all have a tendency to project our own views onto the population as a whole. This is particularly true when one tries to interpret the reasons behind a position with which one agrees. If one feels that American involvement in Vietnam was a mistake and then reads that two-thirds of the population also says that it was a mistake, it is quite natural to assume the reasons are the same in both cases. But of

[7] The other alternative is structural analysis of a large set of interrelated closed questions, as is done insightfully by Modigliani (1972) on poll data from the Korean War period. Ideally both approaches should be used In the present case, where we are attempting to discover basic frames of reference, it seems to me, as it did to Robinson and Jacobson (1969), essential to be able to draw on relatively unprompted verbalizations by respondents. Of course, both these approaches deal with overt "reasons" and "goals," and still further analysis of social and psychological motives is possible; some steps in this direction are indicated later.

course this need not be true, any more than it is true that votes for a Democratic presidential nominee by Mississippi whites and New York blacks spring from the same motives and concerns.

A BROAD HYPOTHESIS

In the summer of 1971 the Detroit Area Study included an open question to provide some insight into the basis of public thinking about Vietnam. Before reviewing those data, let us summarize in the form of a general hypothesis the argument developed thus far. We have seen that public opinion has been turned against the war mainly by reversals such as the Tet offensive. It is also clear that a good part of the public now opposed to the war is also opposed to the antiwar protests and, presumably, the beliefs that they symbolize. Finally, we have found that the larger public opposition to the war includes a substantial proportion of people who are low in education, not very interested in the war, and about as likely to be older as to be younger.[8] What these several pieces of evidence suggest is that much of the disenchantment with the war registered in public opinion polls is of a purely pragmatic character, with little or no concern over the morality of employing American power in Vietnam. Disenchantment with the war is based on our visible lack of success in winning it. Mounting American casualties, the failure of so many optimistic predictions by American leaders, the surprising resilience of enemy forces—all these factors have taken their toll in supporters of the war. More and more Americans now think our intervention was a *military* mistake, and want to forget the whole thing. This explains why the Tet offensive had such a disastrous effect on public opinion, while the My Lai massacre caused hardly a ripple in the polls.

We can also understand in these terms why almost *any* administration action that seems to point toward an end to the war wins at least a brief rise in public support. When American planes first crossed the North Vietnam border, the public hoped that a serious blow had at last been struck against enemy sanctuaries. We lack adequate survey data from that period, but what data we have suggest wide support for the bombing. Five years later, when American troops crossed the Cambodian border, a less optimistic public still showed initial acceptance of another president's claim to be reaching important enemy sanctuaries.[9] In between and subsequently,

[8] On "interest" in the war, see Converse and Schuman (1970) and, using earlier data, Verba et al (1967).

[9] At the time of the Cambodian incursion (May 1970), about 50% of the public regarded our original intervention in Vietnam as a mistake. But a very similar question asked by Harris (1971, p. 124) about Cambodia in May 1970 produced only 39% who thought this new military action was a mistake. By July 1970, even this

almost every action that promised to hasten the end of the war, retreats as well as offensive actions, has won support from a large part of the public.

The distinction between moral and pragmatic opposition to the war is blurred by the policies of troop withdrawal, since these tend to be supported by both viewpoints. But the invasion of Cambodia could appeal only to the pragmatic opponent of the war. Provided he accepted the president's assurance that "cleaning out the sanctuaries" would speed troop withdrawals, the pragmatic objector could support the invasion. To the moral opponent of the war, however, the Cambodian intervention, no matter how limited in time or successful in outcome, meant that the destruction of war was now to be brought to Cambodian villages. On this issue, on the more recent American bombing, and perhaps on issues still to come, the different sources of opposition to the war lead to sharply different stands even among the majority who want an early end to American involvement

METHOD AND SOURCE OF DATA

Let us turn now to the reasons that the general public gives for opposing the war. From April through August 1971, the Detroit Area Study interviewed an area probability cross-section sample of 1,881 persons, 21 years old and over, in the metropolitan Detroit area.[10] The interview included the Gallup "mistake" question discussed earlier; it was followed by open probe questions to those who said they thought our sending troops to Vietnam was a mistake. The probes were simply: "Why would you say it was a mistake?" and "Is there any other reason why you think it was a mistake?" The instruction to interviewers was to be completely nondirective but to encourage full responses. The resulting verbal data from the 1,263 respondents who said "yes" to the closed question have been coded in terms of 10 broad themes developed partly on the basis of theoretical expectation and partly after carefully reviewing 100 responses chosen at random. In the final coding, each of the 1,263 responses was coded zero for a particular theme if that theme was not mentioned; if the theme was mentioned, the response was further categorized in terms of the way the theme was treated. Table 1 presents the five themes that are most relevant for our analysis, along with the marginal percentages for the metropolitan

39% had shrunk to 24%, presumably on the basis of reports that the incursion had ended and had been relatively successful Thus doubts about Vietnam did not prevent hope for the success of a similar military operation in another country.

10 A report on the sampling design for the 1971 survey can be obtained from the Detroit Area Study, University of Michigan, Ann Arbor. The geographic area covered is the Detroit Standard Metropolitan Statistical Area (SMSA), minus the city of Pontiac and the outlying semirural areas of the three-county area; it includes about 85% of the SMSA population.

TABLE 1

REASONS AGAINST U.S. INTERVENTION: MAJOR THEMES CODED (WITH PERCENTAGES FOR DETROIT SAMPLE)

	Theme	%
I.	U.S. Not Winning War:	
	0 Theme not mentioned	66
	1. The war is unwinnable Example: "It can't be won militarily; it's guerilla warfare, not like World War II or Korea."	10
	2. We are not trying to win the war "Win or get out."	8
	3 We are not winning (stated as a fact with no additions) "We're just getting beat like crazy."	1
	4. The war is not ending (low priority relative to 1, 2, 3) "The war just goes on and on"	16
	Total	101
	N	(1,263)
II	People Killed or Injured by the War:	
	0 Theme not mentioned	58
	1 American soldiers killed or injured "So many boys being killed"	28
	2 American soldiers hurt in other ways "All those soldiers getting the dope habit"	2
	3. People killed or injured: identity ambiguous "So many innocent lives have been taken"	7
	4 Both Americans and Vietnamese explicitly mentioned; objections to all war "I hate violence" "Too many Americans and Vietnamese killed"	3
	5 Vietnamese killed or injured (includes references to any Vietnamese, on either side, either civilian or soldier) "Thousands of Vietnamese killed by the mass bombing"	0
	6 Vietnamese people hurt in other ways "We make racketeers out of the people and prostitutes out of the women."	1
	Total	99
	N	(1,263)
III	Loss of U.S. Resources:	
	0 Theme not mentioned	80
	1 U S resources wasted: no mention of alternative social uses "It's ruined our economy."	9
	2 U S. resources wasted: explicit mention of alternative social uses "We send money there and there's poverty here."	3
	3. War causes polarization in U S. "All the young people are turning against the country"	4
	4. We have enough problems of our own to take care of (vague; low priority) "Enough problems here at home"	4
	Total	100
	N	(1,263)
VII.	Vietnam War Is Internal Conflict	
	0. Theme not mentioned	54
	1. It is a civil war (codes only clear references to *civil* war)	5

TABLE 1 (*Continued*)

Theme	%
2. Vietnamese don't want us there	5
"The Vietnamese don't care who wins, just want to be left alone"	
3. The war is the Vietnamese responsibility, not our war	16
"Let them fight their own war."	
4. Our intervention worsened the conflict (low priority)	1
"We changed a small war into a bigger one."	
5 Shouldn't meddle in other people's business (low priority)	19
"Too messed up . . . we should not get involved in other people's troubles"	
Total	100
N	(1,263)
VIII. U S Goals Morally Questionable	
0. Theme not mentioned	89
1. U S motives wrong or questionable	3
"Our efforts at world domination are subject to question"	
2 We shouldn't force our way of life on Vietnam	6
"Who are we to say what is the right way there."	
3 North Vietnamese or Vietcong justified	1
"North Vietnamese form of Communism is the best way for them."	
4 The war is immoral or wrong; no further explanation (low priority)	1
Total	100
N	(1,263)

Detroit sample.[11] The themes were not mutually exclusive; hence a response could be coded other than zero on as many of the 10 themes as were appropriate, although *within* a given theme a response could be coded in one subcategory only.

The Detroit marginal percentages in table 1 can be inspected in light of the theoretical expectations developed earlier in this paper. Content analysis of speeches and writings by antiwar protest leaders would also be useful as a basis for comparison. For our present purposes, however, we will make use of responses to the same closed and open questions by students in three sociology classes at the University of Michigan: an intro-

[11] The remaining five themes, and percentage coded as mentioning each, were IV Vietnam Not Important to American Interests, 28%; V. Okay to Intervene, But Handling of War Incorrect, 15%; VI Entry into War Not Procedurally Correct, 15%; IX The War Is Confusing, 11%; X. Problems with South Vietnamese Government or People, 6%. In general, these themes tend either to duplicate or to provide additional support for the results presented in the text. A copy of the complete set of coding instructions for all 10 themes, together with the results of check coding, can be obtained from the Detroit Area Study Coding was carried out by professional Survey Research Center coders

ductory class consisting almost entirely of freshmen; the same course but with a majority of sophomores; and a more advanced concentration course for juniors. The combined sample (initial $N = 278$, reduced to 236 who answered "yes" to the closed question), while obviously not an adequate representation of the university, provides a useful contrast, as we will see. In one sense it is too conservative a sample, since it seriously underrepresents juniors, seniors, and graduate students who have been most fully exposed to the prevailing university views on the war.[12] Exactness is not absolutely essential here because we use the sample mainly to bring out broad differences between the general population and at least a core of students at the university. It can safely be assumed that the students and faculty members who actually engage in antiwar protests would show more sharply the same trends as our classroom samples.

It would be misleading to proceed as though the open-ended responses and code summaries can provide a definitive, completely objective, or simple test of the moral-pragmatic distinction I have proposed. Instead we will use the data in table 1 on an exploratory basis both to evaluate important aspects of that distinction and to illuminate public thinking about the war. Let us begin with the evaluative emphasis, looking at the themes that provide an indication of the degree of moral concern about the war in the general (Detroit) population. The results for the student and Detroit samples on the three most relevant themes are summarized in table 2 [13]

[12] All the variables reported below that have been examined *within* the student sample show an accentuated difference for the more advanced classes This is also reflected in answers to the initial "mistake" question. 85% of the freshman class regard the war as a mistake, 88% of the mainly sophomore class, and 98% of the junior class On Theme VIII, the percentages offering moral criticisms of the United States are 22%, 38%, and 49%, respectively, for the three classes.

[13] Student responses were coded nonzero on an average (mean) of 3 0 themes, as against an average of 2.3 for the Detroit sample By multiplying the Detroit nonzero percentages by 1 3, they can be adjusted upward to a level that "corrects" for this difference in total nonzero responses. The correction is relevant only where the zero category is included in a comparison (table 2B below, but not 2C or 2A), and even then has only minor effects on our results Since the correction itself involves some questionable assumptions and other problems, our tabled data are in their original form However, "corrected data" were looked at, and χ^2's calculated on the basis of such data, in all relevant cases below; in none did an appreciable change result, as indeed is evident from the very large size of most of the χ^2 statistics presented below. We should also note that the student responses differ in several other ways from the Detroit interviews. They were obtained at the beginning of 1972, in self-administered form in classrooms, without the context of other questions, and at the request of a faculty member (rather than a more neutral interviewer) It is doubtful that any of these differences affected the *content* of the responses seriously, though the differences may account for the greater quantity. We will see below that the main Detroit vs student differences are replicated to a significant degree *within* the Detroit sample when younger college-educated respondents are singled out for attention. Other checks on the results will also be presented.

TABLE 2

COMPARISONS OF GENERAL (DETROIT) AND STUDENT SAMPLES ON THREE THEMES

	Students (%)	Detroit (%)
A. Theme II. Identity of People Killed or Injured.		
Americans only (1, 2)	15	73
Both (3, 4)	75	24
Vietnamese only (5, 6)	10	3
Total	100	100
N	(147)	(525)
B Theme VIII: U S Goals Morally Questionable		
Theme not mentioned (0)	65	89
Theme mentioned (1–4)	35	11
Total	100	100
N	(236)	(1,263)
C. Theme VII. Vietnam as Internal Conflict:		
They cause us trouble (3, 5)	43	84
We cause them trouble (2, 4)	57	16
Total	100	100
N	(90)	(518)

NOTE For full description of themes, see table 1 Note that A and C here are based only on those who mention a particular theme, while B compares those who mention a theme and those who do not All three panels show relationships statistically significant at $P < .001$, using χ^2 and 3, 2, and 2 df, respectively.

COMPARISONS OF GENERAL POPULATION AND STUDENTS

Theme II, People Killed or Injured by the War, is one of the two most frequently mentioned by both samples, perhaps partly because of the wording of the original closed question, but even more likely because of the salience of the theme to any question about the war. Students show this concern to a greater degree than does the general public (62%–42%, $\chi^2 = 34.6$, 1 df, $P < .001$), but the more important differences have to do with the types of mention, as shown in table 2A. A primary focus of the college-related antiwar movement has been on the destruction wrought on Vietnam and the Vietnamese by American military technology. Even those moral critics of the war who grant some legitimacy to American political goals argue that the costs to the Vietnamese have long since exceeded any possible gain to them. From this standpoint, the American military effort is well symbolized by a U.S. officer's explanation at Ben Tre during Tet in 1968: "It became necessary to destroy the town to save it" (Oberdorfer 1971, p. 184). Our research question, then, is the extent to which concern for Vietnamese suffering shows up in the answers of those members of the general public who oppose the war. We see in table 2A that, of those Detroit respondents who oppose the war and who mention lives

lost or injured as a reason for their opposition, nearly three-quarters refer *only* to American soldiers. The students, on the other hand, are much more likely to refer to both Americans and Vietnamese and also more likely to refer to Vietnamese only. Again we must note that even the students reflect only imperfectly the emphasis of the humanitarian part of the antiwar movement. But they reflect it to a much greater degree than does the general public.

Theme VIII covers a more political type of antiwar criticism, one which centers not on the destructive nature of the war, but rather on the motivations for, and goals of, American policy in Vietnam. The category includes accusations of imperialism, support for the North Vietnamese, and more general criticisms of the war as "immoral." We would not expect anything approaching consensus on this among students, but in fact more than a third do touch on such a theme, as shown in table 2B. In the Detroit sample, however, only one out of nine persons gave a response classified anywhere under this theme. For the general public, opposition to the war seldom entails a political-moral criticism of American goals in Vietnam.

Theme VII provides a more subtle distinction between moral and pragmatic concerns. The theme as a whole deals with emphasis on the Vietnam war as an internal conflict, but there are two ways of looking at this. The one represented by categories 3 and 5 focuses on our staying out of "their troubles." "Let them fight their own wars" is the epitome of this outlook. The other point of view, categories 2 and 4, carries the assumption that either the Vietnamese do not want American involvement or such involvement only makes the war worse for Vietnam. (Category 1, "civil war," probably belongs with the second point of view, but we omit it as somewhat ambiguous.) In other words, the first perspective on the war is strictly in terms of American interests and concludes that "they cause us trouble"; the second perspective is at least partly in terms of Vietnamese interests and concludes that "we cause them trouble." Summarized under these rubrics, table 2C shows that Detroiters who mention this theme at all do so overwhelmingly in terms of "they cause us trouble." Students, however, are much more likely to place the emphasis on "we cause them trouble."

Thus on all three of these themes we find sharp differences between the general population and the student sample. If we are correct that a sample of students who actually participate in antiwar demonstrations would be still more different from the Detroit population, we begin to get some measure of the gap between the campus-based protests and the public disenchantment with the war reflected in national surveys. It is interesting that this gap itself is validated in a sense by our code, as can be seen from Theme III, category 3: "War causes polarization in U.S." This category is mentioned infrequently by the general population (4%), but 16% of the students refer to the fact that the war has created polarization within

America.[14] Perhaps for much of the Detroit population polarization is not salient because protesters are perceived as deviants rather than dissenters. Students, on the other hand, personally experience the conflict between the university climate of opinion and that in their homes or hometowns.

Some modifications in the moral-pragmatic distinction are required by findings on other themes. Theme I, U.S. Not Winning the War, represents the pragmatic position in perhaps its purest form, namely, that the war is a mistake because we are not winning it, cannot win it, or have not tried to win it (see table 1).[15] Approximately a third of the Detroit respondents give this response, three times more than offer a specifically moral critique (Theme VIII), it is true, but still far less than unanimity. Moreover, 29% of the students are also coded in Theme I categories, indicating that students are nearly as likely to give such a pragmatic response as are members of the general population. It is probable that this is the case generally: what are distinctive are moral types of responses, while pragmatic reasons are given by all groups who oppose the war. Only when the two positions are incompatible within a particular theme will students and the general population differ greatly. One other theme given frequently by *both* students and general public is Theme IV-4: "We gain nothing from the war" (not shown in a table). This is coded for 20% of the students and for 22% of the Detroit sample, and indicates the general lack of clarity about American aims and purposes in Vietnam. Finally, an unexpected finding occurs with Theme X-1: "Negative characteristics of South Vietnamese government" (not shown in a table). We had expected this to be mentioned with some frequency by the general population, since it is often referred to in the mass media, but in fact it is hardly mentioned at all (3%). Students are significantly more likely to focus on negative characteristics of the South Vietnam government (10%), suggesting that this complaint about our involvement tends to appeal mainly to those influenced by a general political-moral criticism of the war.

[14] This category against all others (including zero) yields a χ^2 of 49.8, 1 df, $P < .001$, for students vs. Detroit. Unlike the comparisons in table 2, this comparison is strictly post factum, but its significance is so high as to make replicability likely.

[15] The subthemes obviously differ greatly both in their sophistication and in their implications for action. "The war is unwinnable" includes the type of judgment finally made by those Pentagon officials and advisers who, having first participated in the escalation of the war, later sought to deescalate it (see Hoopes 1969) "We are not trying to win the war," on the other hand, is a pure hawkish response identified with the military's push for more extreme bombing and related measures The remaining two categories suggest less a policy point of view than a matter-of-fact if weary observation. Despite these important differences, all four categories must be described as pragmatic in terms of our present frame of reference.

ANALYSIS WITHIN THE GENERAL POPULATION: SEX, RACE, AND SES

The Detroit sample can be further broken down in several useful ways. As was mentioned earlier, public opposition to the war in surveys over the past seven years has been associated with lower education and to some extent with older age. Opposition has also been more characteristic of women than of men by a small degree, and of blacks than of whites by a substantial margin. As table 3 shows, both the Gallup national data of

TABLE 3

PERCENTAGE BELIEVING THE UNITED STATES MADE A MISTAKE IN SENDING TROOPS TO VIETNAM, BY SEX, RACE, EDUCATION, AND AGE

	Gallup National Results (May 1971) (N = 1,500+)	Detroit Area Study Results (April–August 1971) (N = 1,881)
Sex.		
Male	65	66
Female	72	72
Race.		
White	67	68
Black*	83	82
Education.		
College	66	70
High school	67	68
Grade school	75	73
Age		
21–29	63	68
30–49	67	66
50 and over	73	74
Total	68	69

NOTE.—Percentages calculated after removing missing data (11% reported by Gallup, 3.3% by Detroit Area Study)
* Includes other nonwhites for Gallup only

May 1971 and our Detroit sample of summer 1971 continue to display these sex and race relationships with almost identical percentages. Gallup also shows small but clear age and education relationships, while our Detroit sample reveals less consistent associations for both these variables.[16] In any case, it is useful to know how all four of these basic back-

[16] When race is controlled, the Detroit sample reveals the same relationship to age for whites that Gallup reports for the nation as a whole. This is a reasonable control, since the percentage of blacks in our Detroit sample is twice that at the national level and therefore prevents an exact comparison. (From this the reader will infer correctly that the relationship of age to opposition to the war is reversed for blacks. younger blacks in the Detroit sample are more likely than older blacks to regard our intervention as a mistake) However, with or without the control for race, we do not obtain for Detroit the usual (or any other) association between support of the war and education.

ground characteristics relate to reasons for being against the war.[17] At least three, it may be noted, identify groups excluded from political dominance: blacks, women, and low-educated persons.

Let us begin with education and age, two variables that are usefully treated together, and with the focus on the white subsample where the number of cases is large enough to allow for more detailed analysis. The main findings here, as shown in table 4, fit well with those reported earlier

TABLE 4

A PERCENTAGE MENTIONING U S MOTIVES MORALLY QUESTIONABLE (THEME VIII) BY EDUCATION AND AGE

	Education				
Age	0–8	9–11	12	13–15	16+
21–29	...(5)	4 (28)	3 (67)	12(49)	28(36)
30–49	0(21)	4 (75)	8(147)	17(76)	24(66)
50+	8(90)	5(100)	12 (89)	13(47)	21(38)

B PERCENTAGE MENTIONING AMERICAN SOLDIERS KILLED OR INJURED (II-1, 2) BY EDUCATION AND AGE

	Education				
Age	0–8	9–11	12	13–15	16+
21–29	...	50	40	14	17
30–49	38	28	35	21	20
50+	40	33	26	25	26

NOTE —Results are for whites only. The figures in parentheses indicate the base *N* on which each percentage is based. The same bases apply in both panels of the table. The cells based on only five cases are omitted as unstable; the frequencies are zero in A and one in B

for students versus the general population. For example, Theme VIII, U.S. Goals Morally Questionable, produces a strong positive relationship to education, with a quarter of the college graduates voicing some moral criticisms of U.S. motives or actions, but decreasing proportions doing so at lower educational levels. Age may act as a conditional factor here, with the gap in mention of this moral theme somewhat greater between high and low educated for the young than for the old. That is, among young people 21–29 who oppose the war, those with college education are fairly

[17] There are slight differences in the average number of codable (nonzero) responses given by different population subgroups At the extreme, those with grade school education (0–8) give an average of 2.16 nonzero responses; those with some college (13+) give an average of 2.44 nonzero responses—a ratio of only 1 1 This difference is too slight to affect the results presented below. Black-white, male-female and age differences are even smaller on this response count.

likely to do so in terms morally critical of government actions, while those with less than high school education are very unlikely to do so. Complementing this, we find that on Theme II, People Killed or Injured, those young opponents of the war without college education are considerably more likely to mention American soldiers being killed or injured (and less likely to mention Vietnamese) than are those with at least some college training—perhaps reflecting the fact that there is not only a social class difference in sensitivity to moral issues but also a social class difference among young men in the risk of entering the army and being sent into combat. These data support our earlier finding of a gulf between college students and the general population in their reasons for opposing the war, and also reinforce the point sometimes made that the gulf is not only between young and old but between those young people with college education and those without it.[18] We are also reminded that moral reasons for opposing the war (as well as principled support for it) may be easier to elaborate when one is not directly in the line of fire.

The less educated, therefore, and especially less educated youth, are particularly likely to interpret their opposition to the war in terms of the danger to American lives. Since older Americans tend to be low in education, this also helps explain the special opposition to the war of older people. However, age as such is associated with still another type of opposition, labeled earlier as "they cause us trouble" (Theme VII-3, 5). This

[18] The methods developed by Goodman (1969, 1972) for analysis of multivariate contingency tables were applied to the results in table 4B. For full table (including the five cases omitted in percentaging), only the relationship between education and mention of American soldiers is significant ($\chi^2 = 21.2$, 4 df, $P < .001$; with age partialed out, $\chi^2 = 32.5$, 4 df, $P < .001$). Age is not significantly related to mention of American soldiers ($\chi^2 = 0.8$, 2 df), nor is there a significant three-way interaction ($\chi^2 = 11.6$, 8 df, $P > .10$). However, since the interaction expected on the basis of our earlier student versus Detroit findings was specified in terms of young college-educated persons, the problem was run again with age and education each reduced to two categories The table below presents the observed percentages on the basis of this specification, along with, in brackets, those expected on the hypothesis of no three-way interaction. (The appropriate base N's can be constructed from table 4.)

Age	Education: 0–12	Education: 13 and Over
21–29	42 [36]	15 [22]
30 and over	33 [34]	22 [20]

The predicted interaction does occur ($\chi^2 = 4.7$, 1 df, $P < .03$ for rejection of two-variable model). The largest discrepancies between observed and expected frequencies of mention of American soldiers are located among those 21–29 years old, indicating that the young college-educated differ not only from the rest of the population generally but especially from the noncollege portion of their own cohort. I am indebted to Otis Dudley Duncan for pointing out to me the particular relevance of Goodman's approach for this problem; Davis's (1972) exposition also proved helpful.

stance should perhaps be relabeled "traditional isolationism," for it focuses on the avoidance of intervention into troubles elsewhere. The percentage giving this response is directly associated with age, but in a single-step threshold fashion:

Age in Decades					
29–29	30–39	40–49	50–59	60–69	70 and Over
32	29	31	42	40	42

Those over 50 are a third again as likely as younger persons to voice this sentiment, but there is little variation within either of the age categories created by the division at 50. The relationship continues to hold when education is controlled, except that the rejection of isolationism is strongest in the 30–49 age range. This is exactly the generation that came to maturity between the beginning of World War II and the beginning of serious frustration over Vietnam—that is, the generation most exposed to what might be called successful "military internationalism" on the part of the United States. Presumably it is this age group that finds especially resonant administration appeals referring to "Munich" and to the early episodes of the Cold War. In any case, whether or not this generational interpretation is correct, we see that the *older* age groups that have been disproportionately opposed to the war have often been drawn to that position on the basis of traditional isolationist sentiments. We assume that the failures of intervention in Vietnam have reinforced these sentiments, although we lack trend data to demonstrate such reinforcement.[19]

The persistence of both sex and race differences in poll data on the war over the last seven years has been interpreted as evidence that at least two groups—blacks and women—have special reasons for their opposition. It is easy to hypothesize that these reasons fit under the several categories that operationalize moral reservations or criticisms. In the case of women this could involve a less aggressive and greater humanitarian attitude. Black opposition, on the other hand, could reflect disenchantment with American society generally and therefore a greater willingness to criticize the war in moral or ideological terms. We find some support for both these expectations in our thematic data, but important qualifications are needed as well.

Considering sex differences first, table 5 shows that women are more

[19] One other result involving education that is worth noting is a positive association between number of years of schooling and belief that the war is "unwinnable" (I-1). Percentages coded into the latter category are. 4% (grade school), 7% (some high school), 10% (high school graduates), 15% (some college or above). The finding supports our earlier note about the sophistication of this point of view. The association involves only education and not age.

TABLE 5

PERCENTAGES BY SEX ON SELECTED ANTIWAR THEMES AND SUBTHEMES

	Men ($N = 535$)	Women ($N = 728$)	Difference
Theme I: U.S. Not Winning War			
We are not trying to win the war (2)	11	5	6*
The war is not ending (4)	13	18	−5*
Theme II: People Killed or Injured	33	48	−15**
Americans only (1 and 2)	22	37	−15
Both (3 and 4)	9	10	a
Vietnamese only (5 and 6)	2	1	a
Theme VIII: U.S. Goals Morally Questionable	15	7	8**

a Less than 2% difference.
* $P < .05$, using χ^2 for category I-2 versus remainder of Theme I (df = 1). $P < .01$ for category I-4 versus remainder of Theme I (df = 1).
** $P < .001$, using χ^2 for zero versus nonzero categories (df = 1).

likely than men to mention "People killed or injured" as a reason for opposing the war, but that this difference is entirely accounted for by the "Americans only" category. Men and women do not differ at all with regard to mention of Vietnamese deaths or suffering. Thus the greater concern of women for the pain of war seems to be channeled wholly along national lines. On other themes, our general finding is that men are more critical of the war effort in *all* ways, both moral and pragmatic. Men are more likely to complain that "we are not really trying to win the war"—a "hawk" type of response—but men are also more likely than women to question the morality of U.S. motives and actions in Vietnam.[20] Women are more apt to phrase their opposition in more passive ways, for example, that "the war goes on and on" (I-4). These findings help explain why the sex difference in opposition to the war has never seemed to translate well into political actions. Despite their concern for American lives lost in war, Detroit women are less rather than more critical of the policies that support and guide the war effort.

The black-white difference on the basic "mistake" question is the largest in table 3, and this finding of greater black opposition has held consistently over the course of attitude surveys on the war. Moreover, a number of student responses, self-identified as black, offer criticisms of the war in clear racial terms: that the war is racist and genocidal, that the money should be spent at home on urban problems, etc. Despite this indirect evidence that blacks may be ideologically more opposed to the war than whites, the Detroit black sample of adults gives only a little evidence

[20] These conclusions are *not* changed when age and education are controlled. For example, considering only the youngest and most educated respondents—those 21–29 with 13 or more years of school—26% of the 53 men question U.S. motives or actions (Theme VIII), as against only 6% of the 32 women.

TABLE 6

PERCENTAGES BY RACE ON SELECTED ANTIWAR THEMES AND SUBTHEMES

	Black (N = 322)	White (N = 941)	Difference
Theme I. U.S. Not Winning War (1–4)	20	39	−19*
Theme II: People Killed or Injured	42	42	a
Americans only (1 and 2)	31	30	a
Both (3 and 4)	10	10	a
Vietnamese only (5 and 6)	1	2	a
Theme III: Loss of U.S Resources	22	20	a
Explicit mention of alternative uses (2)	4	3	a
Theme VII: Vietnam War Is Internal Conflict	52	44	6
They cause us trouble (3 and 5)	42	32	10*
We cause them trouble (2 and 4)	5	7	a
Theme VIII: U.S. Goals Morally Questionable ..	10	11	a
Theme IX. The War Is Confusing (1 and 2) ...	18	6	12*

[a] Less than 2% difference.
* $P < .001$, using χ^2 for categories shown versus all other categories of same theme (df = 1 in each case).

of being more highly motivated by moral sentiments than are whites. As table 6 shows, blacks as a group do not differ from whites in their distribution of responses to Theme II, People Killed or Injured, or to Theme VIII, U.S. Goals Morally Questionable. They also do not differ significantly on a theme (III-2) which concerns alternative social uses of money and resources now being spent in Vietnam, although this is a point often made by black leaders. Blacks do differ, however, quite substantially on Theme I, U.S. Not Winning War, with 19% *fewer* responses here than reported by whites, the reduction being distributed evenly over categories 1, 2, and 4. In other words, blacks are much less concerned than whites about the lack of "victory" in Vietnam, and therefore in this sense black opposition to the war seems less pragmatically based than is the case for whites. But this deemphasis on pragmatic opposition does not appear to be translated into a more positive critique of the war. The 19% difference on Theme I is not compensated for elsewhere in any single category; it generally appears to be reversed in the categories we have called, "They cause us trouble" (Theme VII-3, 5), as well as on other categories indicating confusion over what the war is about (especially Theme IX). Together these findings suggest a picture of the war as a distant and unclear set of troubles belonging to someone else—an isolationist trend of thinking based on low interest in the war, rather than on conscious moral opposition to it.[21]

[21] Detailed internal analysis of the black sample is difficult because of the small number of cases. However, certain findings can be noted. On the initial "mistake" question,

DISCUSSION

When the evidence from both past opinion surveys on Vietnam and the present Detroit study is drawn together, the broad distinction between

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blacks at *all* age and educational levels are more apt than comparable whites to consider intervention to have been a mistake; however, the differences are greatest among persons in their twenties regardless of education. This may suggest that younger blacks have a distinctive set of reasons for opposing the war, but in fact nothing of this sort emerges. On the contrary, we do find that older and less educated blacks report more "confusion" about the war (Theme IX) than do comparable whites, but there are no uniquely emphasized reasons given by younger blacks in our sample In sum, we are able to "explain" the greater opposition to the war among both older and less educated blacks, but there seem not to be equally salient patterns among younger, more educated blacks that distinguish them from comparable whites The clearest trend is for young college-educated blacks to mention the waste of money in Vietnam (III-1), but this is a very small subsample ($N = 19$) and the import of the response is not clear; Theme III-2, dealing with alternative uses for the money at home, is *not* emphasized. When sex is controlled, differences emerge for blacks similar to those for whites. Unfortunately we are not able to control age, education, and sex simultaneously within a cross-tabular framework allowing a search for interaction It is also possible that black responses are being obscured by race-of-interviewer effects (Hyman 1954; Schuman and Converse 1971). Our sample design included a systematic variation by race of interviewer, and we do find that black respondents are significantly ($P < .05$) more likely to question U.S. goals (Theme VIII) when being interviewed by blacks (13%) than by whites (6%). The association is not strong, however, and even if we substitute the higher percentage (13%) into table 6, the difference between blacks and whites on this theme remains trivial. No other themes show significant differences by race of interviewer; there are some trends that are of theoretical interest (the largest involving greater use to black interviewers of Theme IV-4 "We gain nothing from the war"), but again they are too slight to change the basic picture presented in table 6 Moreover, a special recoding of a subsample of 110 black responses to uncover subtle racial references possibly unnoticed in our thematic analysis produced only one such reference. One final piece of information that is suggestive in interpreting the racial differences comes from an additional coding of the open Vietnam responses. When respondents referred to the U.S. government indirectly by use of a pronoun, we coded whether the pronoun chosen was "we" or "they" Blacks are considerably less likely to use "we" and more likely to use "they" than are whites:

	Blacks	Whites
"We"	59%	85%
"They"	41%	15%
	100%	100%
	(219)	(709)

NOTE.—$\chi^2 = 67.9$, df = 1, $P < .001$.

This racial difference holds up well when age and education are simultaneously controlled (mean racial difference over nine categories: 22%). However, it is greatest among young, well educated blacks; indeed, use of "we" generally increases with increased education among both blacks and whites, *except* among blacks 21–29, where the direction is reversed. The exact meaning of these findings is not entirely clear, but they probably signify a sense of distance from the exercise of governmental power on the part of blacks, though how much of this is passive and unconscious, as against actively alienated, remains to be determined.

moral and pragmatic types of opposition to the war remains a persuasive one. The college-based protest has been led almost entirely by spokesmen presenting the moral critique, but much of the public opposition to the war flows from quite different sources. These have to do primarily with the long and frustrating nature of the war but also draw on other closely related themes, of which the two most important are probably the costs in American lives and the lack of clarity about the goals of the war. A very pragmatic current of isolationism is also involved, symbolized by the phrase: "Let them fight their own war."[22]

The moral-pragmatic distinction does not, of course, correspond exactly to the difference between major universities and the rest of the population One will find both types of opposition in both settings—though, as we have shown, their proportions differ sharply. Nor is it necessary or wise to assume that the distinction represents characterological differences between the campus and the city. We are dealing here with ideology, not with personality. While it is probably true that some of the leaders and participants in the college-based protest movement are motivated by deeply held ethical principles, it would obviously be a mistake to infer individual character directly from verbal reasons for opposition to the war. College students provide moral criticisms primarily because they are exposed to, and learn, such criticisms on campus. In addition, they are intellectually equipped to elaborate their sense of dissatisfaction with the war, and to turn personal concern about participating in it into a critical examination of its goals—what Weber meant by rationalization, which is far more than merely "explaining away" something distasteful. Our aim here has been an analysis of the content and social bases of antiwar sentiments and ideology, not an attempt at delineation of personality differences.

From a policy standpoint, the main overall implication of our argument is that the president has never had much to fear directly from the college antiwar movement, because the latter does not speak the same language

[22] One must expect emphases to change somewhat over time as the impact of the war itself changes. Several months after our main field interviewing, we conducted brief telephone reinterviews with a random subsample of 198 respondents Nine of the 10 themes showed a drop in mention, perhaps merely a function of the telephone context, but one showed a rise This was Theme VII Vietnam as an Internal Conflict, and especially the subthemes (3 and 5) we have labeled "they cause us trouble" or traditional isolationism, which increased from 32% to 46% for the relevant reinterview subsample ($N = 119$). The largest single drop in mention (from 42% to 27%) occurred for Theme II: People Killed or Injured. Since the salience of the war itself was decreasing at that point (for example, U.S. casualties had declined to a relatively tiny number), it makes sense that such specific objections were disappearing and being replaced by a hardening of general isolationist sentiment toward a more and more remote "nuisance." These findings reinforce the value of the broad abstractions "moral" and "pragmatic"; the distinction carries the danger of oversimplification, but it also points to more enduring stances toward the war than do most of the specific themes and subthemes.

as the general public. Public disillusionment with the war has grown despite the campus demonstrations, not because of them. The president's primary enemy is and always has been the Viet Cong and the North Vietnamese, for it is their resilience and success that undermine larger public support for the war. The antiwar movement is not wholly ineffective: it influences commentators and columnists, who in turn (but in different words) influence the public. And it provides energy and money in political campaigns. But attempts by moral spokesmen against the war to proselytize the general public directly are likely to fail or even prove counterproductive unless carried out with more skill and less righteousness.

There is another long-term implication to the moral-pragmatic distinction. Our Detroit questionnaire included a question on a possible future Communist-inspired revolution in South America (preceding the Vietnam question by several items). As might be expected, those who regard the present Vietnam war as a mistake are more likely to resist intervening in such a future situation. But our thematic code proves useful in distinguishing further. Of those who are opposed to the Vietnam intervention simply because we are not winning (Theme I), 50% would still intervene in a new South American war. But of those who criticize the Vietnam war on moral grounds (Theme VIII), only 25% would intervene in a new war. The *reasons* people give for thinking the Vietnam war a mistake are linked to their willingness to become involved in future wars of the same general type. We need not pretend to be entirely clear on cause and effect here, but we can insist on the value of understanding more thoroughly not only pro and con positions, but the reasons for them.

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Alienation and Action: A Study of Peace-Group Members[1]

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The relation of social-psychological orientations to overt action is analyzed by examining the pattern of association of a number of scales to membership, degree of activity, and degree of radicalism in peace groups. Four types of alienation are found to be significantly related to social-action patterns, but in different directions. Mode of explaining social events is found to be a crucial underlying orientation. But analysis of recruitment patterns indicates interpersonal and status factors also determine peace-group membership. Biographical data suggest that orientations predisposing to peace-group membership take shape in youth and early adulthood instead of being internalized in childhood.

A fundamental issue in social psychology is the relation of social-psychological orientations to overt social acts. Too often, tests of reliability are tacitly taken as evidence for the predictability of action from personal orientations. However, studies such as those on prejudice and discrimination raise considerable doubt about the direct relation between social-psychological orientations and action (Brookover and Holland 1952; Dean 1958; Linn 1965). Deutscher not only cites empirical evidence but raises a range of theoretical questions about the expectation of a direct relation between expressed attitudes and action (Deutscher 1966). The most recent review of the literature, covering a number of areas, concludes that the review "provides little evidence to support the postulated existence of stable, underlying attitudes within the individual which influence both his verbal expressions and his actions" (Wicker 1969). Situational factors, of both structural and episodic origin, play an important role in shaping action.

But the healthy corrective to the simplistic assumption of the direct predictability of action from attitudes may be carried too far. Sociologists have long insisted that people act in terms of their definition of the situation. While writers such as Goffman show that definitions and norms are often constructed as specific to the immediate episode of interaction (Goff-

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man 1963), Mead's conception of the "I" and the "me" as a miniature system of social interaction implies an autonomy of the self from immediate situational determinants that suggests that the part played in the defining process by more or less stable personality orientations may be considerable (Mead 1934). Moreover, people are not thrust into situations at random. A person selects and is selected for situations in part on the basis of his presumed social-psychological orientations. Continuity in orientation may thereby be reinforced in a way that masks the relative influence of personality and situational factors.

Because of the relative fluidity of social movements, study of recruitment and personal orientations of participants affords an opportunity to sort out some of the influences operating to bring about various kinds of action. In the study reported here, data were collected on the recruitment and on a range of social and attitudinal characteristics of peace-movement participants. Comparable data were also obtained on a sample of nonparticipants. The form of sampling of peace-movement participants also permits examination of intensity of activity and degree of radicalness of activity among peace-group members. Hence, attitudinal data can be related to several dimensions of action. I shall first examine a body of relationships between attitudinal data and peace-movement action and then consider the manner in which situational and structural factors enter into recruitment into peace groups.

PROCEDURE

The samples (with one exception, to be discussed later) were drawn from a suburban university community of about 11,000 during one of the peak periods of peace activity, late summer and fall of 1962. Three organizations in the community were considered peace groups but varied distinctly in degree of radicalness of ideology and action. One was a chapter of the American Association for the United Nations (AAUN), which represented a type of peace activity closest to conventional attitudes and practices.[2] The AAUN supported an international organization of which the United States is a member and, though espousing disarmament and world law, did not strongly criticize American policy or participate in the protests and demonstrations of more radical peace groups. The local chapter considered its function educational and limited its activities to sponsoring rather bland speakers and recruiting members for the national organization.

The second peace group was a chapter of the United World Federalists

[2] The AAUN has subsequently merged with the U.S. Committee for the United Nations, a quasi-governmental organization, and has adopted the name the United Nations Association. However, this merger came well after the present study, and I will use the AAUN name.

(UWF), which focuses on world government but supports most of the principles of the peace movement short of pacifism. While national UWF has had more support from Republicans and businessmen than have other peace groups, the local members and leadership were overwhelmingly Democrats and professionals. Though nationally only slightly more radical than the AAUN, the local UWF group was considerably more so—probably partly because the UWF was for some years the only peace group in the community and hence drew members from the whole spectrum of peace-movement-oriented people. The local group was often vocally critical of U.S. policy and brought in speakers representing a fairly radical criticism. Though operating as a conventional interest group, the UWF chapter employed much more vigorous action tactics than the AAUN—organized telegram and letter writing campaigns, conducted workshops on protest tactics, wrote letters to the editor, etc.

The third group was an unaffiliated group, which will be referred to as the Community Peace Group (CPG). Largest of the three groups, CPG represented a considerably more radical position than the other two. Though leadership was largely nonpacifist, most pacifists in the community belonged to the CPG. The group had the widest range of activities: a newsletter, speakers' bureau, public protest meetings, newspaper ads and petitions, and support of a political lobbyist. Sponsored speakers were often identified with the more radical criticism of American policy. Members often engaged in demonstrations and vigils. Despite considerable membership overlap between UWF and CPG, the former rejected joint activities, implying that the radical image of the CPG would be detrimental to the effectiveness of the UWF.

Thus, the three groups could clearly be arranged by degree of radicalness of ideology and action, from the AAUN, to the UWF, to the CPG. There is no quantitative measure of degree of radicalness, but at least the rank order is very clear. However, because the three community peace groups all represented the relatively moderate portion of the peace movement, a small sample (30) of radical pacifists was obtained from outside the community. In the absence of a definable universe, no systematic sampling procedure was used. However, all the radical pacifists were persons known to have engaged in such tactics as civil disobedience, protest boardings of Polaris subs, lengthy peace marches, vigils, and sailing protest ships into nuclear testing zones. Most subjects were members of one or both of the two then most radical peace groups: the War Resisters League (WRL) and the Committee for Nonviolent Action (CNVA). About half were living on a CNVA training farm developing nonviolent resistance tactics. These activities indicate a radical ideology challenging a major part of American life and/or foreign policy. These radical pacifists had, for the most part, given up on conventional tactics or institutional channels

for attaining their goals. Most had been subject to jail sentences or police harassment. More than half of the radical pacifist sample were full-time peace workers, and all were devoting a higher proportion of time to peace work than almost any member of the three community groups. This sample thus enables us to carry the analysis to a more radical extreme than was afforded by the peace groups in the subject community.

Within the community, two samples were drawn. One consisted of all known members of the three peace groups. Although membership of the three groups totaled 131, because of overlapping membership the sample contained only 95 persons. Only one member refused to complete the questionnaire (she had just disaffiliated). The second sample was a random sample of nonmembers. Approximately one household in every 10 was sampled, with male and female spouse taken alternately (students were eliminated from the sampling procedure). This sample produced 234 usable questionnaires, an 83% return. Areal analysis and known characteristics of those who refused or could not be contacted indicate that elderly people, persons of low income, and, probably, conservatives are somewhat under represented. However, as our purpose is not a characterization of the general population but a comparison with the peace-group sample, these deficiencies are not a disadvantage, since peace-group members were overwhelmingly middle-aged, college educated, and politically liberal.

FINDINGS

We should first examine the sample relations on two scales specific to peace activity: political attitudes[3] and attitudes about foreign policy.[4] Initially, I

[3] The political attitudes scale contained seven items, four of which are given here. Each could be answered on a five-point scale from "strongly agree" to "strongly disagree":

2. The freedom riders have worsened the lot of the Negroes in the South.
3. The drinking water in (university town) should be fluoridated.
4. The U.S. government should provide completely for the medical needs of every citizen over age 65.
6. Atheists should be prohibited from teaching in our public schools.

Gammas were calculated for the relation between the scores on each item and the total scale scores. The γ's for the above items were. (2) .84, (3) .65, (4) .63, and (6) .85.

[4] The foreign-policy attitudes scale contained nine items, four of which are given here These items could be answered on a five-point scale from "strongly agree" to "strongly disagree":

1. Communist China should be admitted to the United Nations.
5. The U.S. government should permit the sale of our surplus grain (at world market prices) to Communist China.
6. The United States should undertake nuclear disarmament unilaterally, regardless of what Russia does
7. The United States should commit itself to developing the United Nations in

will limit attention to the sample of peace-group members and nonmembers drawn from the same community. The comparisons are given in tables 1 and 2. Although the peace-group sample was markedly more liberal in political attitudes and had a markedly more internationalist-prodisarmament orientation in foreign-policy attitudes than the nonmember sample ($\chi^2 < .001$ for both scales), there was considerable overlap between the two samples. On the political attitudes scale (with step 9 being the most liberal), 20% of the peace-group sample were in the sixth or a more conservative step, while 33% of the nonmember sample were in the sixth or a more liberal step, 29% of the nonmembers being in steps 6 or 7, which also contained 39% of the peace-group members. On the foreign-policy attitudes scale (step 9 being most internationalist-prodisarmament), 28% of peace-group members were in step 6 or lower, while 22% of nonmembers were in step 6 or higher, with 20% being in steps 6 and 7, which contained 39% of the peace-group members. Thus, it appears that political and foreign-policy attitudes alone are not sufficient to account for peace-group membership, though their influence, particularly when taken in combination, is very considerable. But we are justified in looking to further influences.

Alienation

Alienation, especially in Marxist theory, has often been held to predispose people toward radical movements. If true, we should expect to find degree of alienation positively associated with degree of radicalness of groups to which people belong, a proposition the present research design can test, since we have five samples running from nonmembers through four increasingly radical peace groups. However, the empirical literature is sparse and, if anything, in conflict with the Marxian hypothesis. Rotter (1963) found that Negro students in a southern Negro college who saw themselves as the determiners of their own fate were more likely than those who saw themselves externally controlled to sign statements committing themselves to more personal and decisive forms of civil-rights action. McDill and Ridley (1962) found that anomic and politically alienated low-status people were less likely to vote in a metropolitan election and, if voting, were less likely than unalienated voters to favor metropolitan government. Horton and Thompson (1962) similarly found that the politically alienated were more likely to vote against local issues. Rose (1962) found that group

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the direction of taking over more and more national sovereignty up to the point where the United Nations has sufficient power to eliminate war.

The γ's for the relation between the scores on these items and the total scale scores were. (1) 90, (5) .94, (6) .57, and (7) .63.

TABLE 1

Distribution of Scores on Political Attitudes Scale[a]

Sample	Percentages in Steps (Nine-Step Scale) 1–4 (Conservative)	5–7 (Moderate)	8–9 (Liberal)	N	Percentage in Median Category and Below	Comparison with Peace-Group Sample Q^b	χ^{2c}
1. Three peace groups	2.2	45.2	52 7	93	8 7	. . .	. . .
2. Nonmembers	23.5	73.1	3 6	222	67 2	.91	*
3 Radical pacifists	0.0	50.0	50.0	30	0 0	. .	. . .

[a] The items in this scale and in each of the subsequent social-psychological orientation scales devised for this study were subject to the criteria in the Murphy-Likert technique for scaling. All of the scales are Likert-type scales, with the scores on the individual items added together to produce the score for the scale. The resulting scores were then divided into equal-interval steps

[b] Yule's Q, calculated on proportions of samples above and below the median category.

[c] χ^2 computed on basis of 50/50 assumption, using proportions of samples above and below median category, $df = 1$.

* $P < .001$.

TABLE 2

Distribution of Scores on Foreign-Policy Attitudes Scale

Sample	Percentages in Steps (Nine-Step Scale) 1–4 (Nationalist)	5–7 (Moderate)	8–9 (Internationalist)	N	Percentage in Median Category and Below	Comparison with Peace-Group Sample Q^a	χ^{2b}
1. Three peace groups	3 3	48.4	48 4	91	13.2	. . .	. . .
2. Nonmembers	42 3	55.3	2 4	215	78 1	.92	*
3. Radical pacifists	0 0	20.0	80.0	30	0.0	. . .	. . .

[a] Yule's Q, calculated on proportions of samples above and below the median category.

[b] χ^2 computed on basis of 50/50 assumption, using proportions of samples above and below median category, $df = 1$

* $P < .001$.

leaders are less likely to be alienated than the general population—but only a fraction of the general population were significantly alienated. Seeman and Evans (1962) found that hospital patients low in alienation have more objective information than alienated patients about the hospital and treatment procedures. Seeman (1963) also showed that alienated prisoners learn less about parole information in reformatories. However, a number of conceptualizations of alienation are found in the literature, with little assurance that the various proposed indexes tap any common vein of alienation (Bell 1966; Nisbet 1966). Seeman (1959) sought to order the many variants of alienation under five types: powerlessness, meaninglessness, normlessness, isolation, and self-estrangement. Since the present study included indexes of these five types, the data provide a test of whether these five conceptions are simply variants of a general psychological orientation or are dimensions that may vary independently. Seeman's definitions and the scales used are as follows.

Powerlessness.—Powerlessness, according to Seeman (1959, p. 784), is "the expectancy or probability held by the individual that his own behavior cannot determine the occurrence of the outcomes, or reinforcements, he seeks." The scale used in the present study involves two aspects that bear on man's power to influence outcomes. The first concerned whether human beings can collectively control their fate or are subject to some natural or supernatural destiny they can little influence. The other dimension deals with powerlessness in the face of controls exercised by elites. The eight questions (to be answered on a five-point scale from "strongly agree" to "strongly disagree") used in the powerlessness scale were:[5]

1. Human nature being what it is, there will always be wars.
2. A person should try to lead his own personal life in as decent a way as he can, but there isn't much one can do about the big problems of the world.
3. There's an order of things in life, and there's not much we can do about our fate.
4. Science has its place, but there are many important things that can never possibly be understood by the human mind.
5. History shows that fundamentally people have always behaved about the same way, and there's not much you can do to change them.
6. The important matters are decided by a small group of powerful people, and there's not much of anything the average person can do about it.
7. It's sometimes worthwhile to try to influence things in your local

[5] The γ's for the relation between each item and the total scale scores were: (1) .78, (2) 79, (3) .84, (4) .67, (5) .85, (6) .60, (7) .77, and (8) .76.

community, but it's a waste of time to try to influence events or policy on the national scene.

8. The people who make the decisions which really matter in our world are beyond the influence of the common man.

Meaninglessness.—Meaninglessness, according to Seeman (1959, p. 786), occurs "when an individual is unclear as to what he ought to believe—when the individual's minimal standards for clarity in decision-making are not met." Meaninglessness, then, is the condition where the individual cannot predict with confidence the consequences of acting on a given belief (Seeman 1959, p. 786). The scale used as an index of meaninglessness is Srole's anomia scale (Srole 1956). Srole uses a 1-0 scoring technique, with only the extreme answer on a five-point range being scored as anomic. Where only extreme responses (strongly agree) are scored as anomic, Srole's scale is, as Meier and Bell (1959) say, a measure of "personal demoralization, utter hopelessness, and discouragement." But responses of "uncertain" or even "disagree" (as distinct from "strongly disagree") are more suggestive of uncertainty about predictions of outcomes on items from Srole's scale like: "Nowadays a person has to live pretty much for today and let tomorrow take care of itself," and "These days a pérson doesn't really know whom he can count on." Thus, in the present study a 1-2-3-4-5 scoring technique was used for the five possible responses. Scored thus, Srole's anomia scale seems a good measure of the degree to which respondents perceive the social environment as unpredictable.[6]

Normlessness.—"The anomic situation, from the individual point of view, may be defined as one in which there is a high expectancy that socially unapproved behaviors are required to achieve given goals" (Seeman 1959, p. 788). Because of the political focus of the study, the scale indexing normlessness was a measure of alienation from the political-economic structure of American society. The six items in the normlessness scale were:[7]

1. It is not so much "what you are" as "whom you know" that determines who gets ahead.
2. America is a land of unlimited opportunity, and people get pretty much what's coming to them in this country.
3. The American system of life is quite sound; the danger lies in extremists of both the Left and the Right.
4. Unfortunately, most people with whom I have discussed important

[6] Though Srole's scale had been already standardized on other populations, it is of interest that the items scaled well on the population used in the present study, with γ's for the relation between individual item and total test score all being between .85 and 98.

[7] The γ's for the relation between individual items and the total test scores were: (1) .65, (2) .64, (3) .68, (4) .47, (5) .74, and (6) .67.

social and political questions just don't understand what's going on.

5. The truth of the matter is that it is big business which wants to continue the cold war.
6. There isn't much hope for reducing the crime rate in the United States as long as we have so much materialism and a competitive economic system.

Isolation.—"The alienated in the isolation sense are those who, like the intellectual, assign low reward value to goals or beliefs that are typically highly valued in a given society" (Seeman 1959, pp. 788-89). Whereas powerlessness and normlessness have reference to the *means* by which goals may be achieved, isolation has reference to the degree of acceptance of the socially approved values or goals in the society. Nettler's (1957) alienation scale was used as a measure of isolation. As Seeman notes, Nettler's scale reflects the individual's degree of commitment to popular culture, including the general values of American society (Seeman 1959, p. 789).[8]

Self-estrangement.—Though expressing dissatisfaction with his conceptualization, Seeman suggests that self-estrangement refers to the individual's feeling of alienation from some explicit or implicit human ideal condition, reflected in the individual's inability to find his activities self-rewarding (Seeman 1959, p. 790). The index for self-estrangement in the present study was an item which asked, "Do you feel that you have fallen short—or might fall short—of your hopes and ideals in any of the following . . ?" There followed a list of activities, such as occupation, marriage, being the kind of human being one wanted to be, enjoying life as much as one hoped, etc. A composite score of approximation of ideals was thus obtained. However, none of the comparisons, even when controlled for various factors, produced significant differences among the various samples. Both because of the absence of differences and because Seeman's conceptualization is not clear, I shall not consider self-estrangement in the remainder of the discussion.

I will first compare the total sample of members of the three community peace groups with the sample of nonmembers. However, because of the great difference between these two samples in political and foreign-policy attitudes, and in order to control for education, social status, and tendency to group affiliation (peace-group members being relatively high in all three), subsamples of nonmembers were also compared with peace-group members,

[8] The γ's for the relation between individual items and the total test score for Nettler's alienation scale had the greatest variability of any of the scales used on the population of the present study, running from .26 to .90, with four of the 17 items having γ's below .50. However, since the scale is widely accepted in the literature and consistently discriminated among the samples in the present study, it was retained in its original form.

using subsamples of persons whose characteristics overlapped those of the bulk of peace-group members. It was not possible to generate a subsample sufficiently large for statistical analysis when controlling all five factors or by using a matching procedure. Hence, four subsamples are reported in the tables that follow. Subsample I controls for political attitudes, containing nonmembers who scored in the most liberal four positions on the nine-step scale, in which 91% of the peace-group members also fell. Subsample II controls for foreign-policy attitudes, containing nonmembers who scored in the most internationalist four positions on the nine-step scale, in which 87% of the peace-group members also fell. Subsample III controls for education (at least some college), political attitudes (steps 6–9), and foreign-policy attitudes (steps 5–9)—80% of the peace-group members falling into the range of all three characteristics controlled for in this nonmember subsample. These three subsamples have the deficiency of having a considerably higher sex ratio than the peace-group sample. Subsample IV controls for social status (by an index using education, occupation, and income), number of group memberships in the community, and foreign-policy attitudes—containing nonmembers who were in the upper three of a five-level social-status scale, who belonged to two or more voluntary organizations, and who were in the 5–9 range on the foreign-policy attitudes scale. Seventy-seven percent of the peace-group sample also met all these criteria. This fourth subsample nearly perfectly matches the peace-group sample in sex ratio, .71 compared with .70. This subsample has the deficiency that its members were more conservative on the political attitudes scale than peace-group members.

Table 3 presents comparisons for the four kinds of alienation. In all four types of alienation, the peace-group and nonmember samples differ significantly, with χ^2s beyond the .001 level. While controlled subsamples somewhat reduce the differences, all differences are significant at the .05 level, and 14 of the 16 comparisons of peace-group sample and subsamples of nonmembers are significant at the .01 level or greater. But a crucial point for the analysis of alienation is that peace-group members and nonmembers do not have the same relative positions with regard to the four kinds of alienation. Relative to the nonmember samples, peace-group members are high in normlessness and isolation but low in meaninglessness and very low in feelings of powerlessness. Most striking is the fact that peace-group members are high in normlessness—that is, alienation from the economic-political structure—but very low in feelings of powerlessness to influence that structure. Indeed, efforts of these people to influence political decisions go beyond simply peace-group membership. A measure of political activity covering a wide range of political action (mostly conventional party activity) showed peace-group members far more active than any nonmember sample or subsample.

TABLE 3

DISTRIBUTION OF SCORES ON FOUR ALIENATION SCALES

ALIENATION SCALE	PERCENTAGE IN STEPS			N	COMPARISON WITH PEACE-GROUP SAMPLE		χ^2 COMPARISONS OF PEACE-GROUP SAMPLE AND SUBSAMPLES[c]			
					Q[a]	χ^{2b}	I	II	III	IV
Powerlessness scale:	1–4 (Low)	5–6 (Medium)	7–10 (High)							
Three peace groups	61.1	30.5	8.5	95	...	...	...	...	...	...
Total nonmembers	18.1	40.0	42.1	233	.78	***	***	***	**	**
Meaninglessness scale (Srole's anomia scale):	1–3 (Low)	4–6 (Medium)	7–10 (High)							
Three peace groups	24.1	52.6	23.2	95	...	...	...	...	...	...
Total nonmembers	13.7	44.4	41.8	234	.47	***	***	***	**	**
Isolation scale (Nettler's alienation scale):	1–4 (Low)	5–7 (Medium)	8–10 (High)							
Three peace groups	25.4	36.9	37.8	95	...	...	...	...	...	...
Total nonmembers	59.2	33.6	7.3	232	.62	***	**	*	*	***
Normlessness scale:	1–2 (Low)	3–5 (Medium)	6–9 (High)							
Three peace groups	23.1	43.2	33.7	95	..	...	...	...	...	...
Total nonmembers	37.8	45.1	17.2	233	.46	***	***	***	**	***

[a] Yule's Q, calculated on proportions of samples above and below median category.
[b] χ^2 computed on basis of 50/50 assumption, using proportions of samples above and below median category, $df = 1$.
[c] All differences are in the same direction as in the comparison between peace-group members and the total nonmember sample. Number of asterisks designates same level of significance as in footnote b. Subsample I controlled for political attitudes. Subsample II controlled for foreign-policy attitudes. Subsample III controlled for education, political attitudes, and foreign-policy attitudes. Subsample IV controlled for social status, group membership, and foreign-policy attitudes.

* $P < .05$.
** $P < .01$.
*** $P < .001$

The data depicting the types of alienation in which nonmembers show greater alienation than the members of peace groups (powerlessness and meaninglessness) reveal that the most marked difference is in powerlessness. Even in the subsamples controlling for political and/or foreign-policy attitudes and for education or social status, 62%–70% of nonmembers scored in the medium or high portions of the powerlessness scale, contrasted with less than 40% of the peace-group members, with the great bulk of that difference being found in the high-powerlessness column. There thus appear to be a considerable number of people of comparable social status in essential agreement with peace-group members on political and international questions but who feel powerless to influence political affairs and do not become involved in organizations such as peace groups or political parties.

Why do people in the same status levels vary so much in feelings of powerlessness? It is a plausible hypothesis that there is an underlying orientation to social phenomena which plays an important role in political action versus inaction: the frame of reference for understanding causal processes in social affairs. Within American culture—and extending deep into academic culture as well—is a fundamental dichotomy in mode of explaining social phenomena. The traditional mode, supported on the one hand by the free enterprise and social Darwinist traditions and on the other hand by much of the humanities, biological sciences, instinct psychologies, and psychiatry in the intellectual world, holds that the biopsychological individual is the causal factor in social occurrences. An armory of instincts, drives, needs, greeds, and passions are assumed to be generated within the individual by his biopsychological makeup. The flow of social phenomena, from this viewpoint, may be attributed to some essentially unalterable biopsychological human nature shared by all members of the species; or the flow may be viewed as resulting from the competitive struggle of individuals differently endowed with variable biopsychological forces.

Alongside this historic theory of social causation has been a sociological mode of conception, rooted in the humanitarian movement of the 18th and 19th centuries and carried on by social-welfare movements, the social-science movement, the politics of the Left, and pragmatic philosophy. The sociological mode sees social causation as lying in the relationships and interactions between people and conceives of the individual as the calculating animal—often misguided by social myths but at least potentially capable of controlling his own destiny in cooperation with other members of his society. The sociological philosophy thus lends itself to optimism about people's ability to influence the course of social events and to rid society of its ills. Biopsychological individualism inclines the believer to pessimism about the possibility of collective intervention in the social process. Whereas the sociological philosophy holds that human—or, more accurately, social—nature can be altered by changing the patterns of social

relations that channel individual action, biopsychological individualism holds that social relations—and in turn the evils of society—are products of the makeup of individuals, and hence intervention in the course of affairs must take the tedious form of direct reformation of the individual.

Of course, these two philosophies could not have persisted side by side in Western culture for so long without interpenetration. Most people probably have an amalgam of elements from each philosophy. But it was hypothesized that most people tend toward one or the other.

To test the hypothesis about the importance of mode of explaining social phenomena, a scale was constructed consisting of six items which could be answered on a five-point scale from "strongly agree" to "strongly disagree":[9]

1. Considering the way Negroes are treated in this country, you can hardly blame them when they sometimes become aggressive or join groups like the Black Muslims.
2. If we had more cooperation and planning in our society, we could produce better human personalities.
3. Most of our social problems would be solved if we could somehow get rid of the immoral, crooked, and feebleminded people.
4. If a person really wants to straighten himself out, he can solve most of his problems by himself.
5. Individualism, the competitive struggle for survival, is the law of nature; so the free-enterprise system is the only way to motivate people to work hard enough to assure progress.
6. There is not much point in trying to solve social problems by monkeying with the social and economic system, because you just can't change human nature.

Table 4 gives the distributions of the various samples on the social explanation scale. The differences are all very large, the association between the peace group and nonmember samples being $Q = .85$, while the range of Q for the subsamples is from .52 to .85 (incidentally, the Q of .61 for the difference between the peace-group sample and the subsample controlling for political attitudes shows that the scale does not simply measure political liberalism, since both samples scored high in the liberal segment of the political attitudes scale). It appears that political and foreign-policy liberals who tend toward a biopsychological mode of social explanation have a sense of powerlessness and manifest a low level of political activity. Political and foreign-policy liberals who tend toward a sociological mode of social explanation have a sense of social power and are likely to be

[9] The γ's for the relation between the individual items and the total scale scores were (1) .79, (2) .46, (3) .66, (4) .77, (5) .84, and (6) .76.

TABLE 4

DISTRIBUTION OF SCORES ON SOCIAL EXPLANATION SCALE

Sample	Percentage in Steps (Nine-Step Scale) 1–2 (Sociological)	3–5 (Mixed)	6–9 (Biopsychological)	N	Percentage in Median Category and Below	Comparison with Peace-Group Sample Q^a	χ^{2b}
1. Three peace groups	57 9	34.7	7 4	95	86 3	..	...
2. Nonmembers	12 5	36.1	51 6	233	34 0	85	**
3. Subsample I, controlled for political attitudes	28 4	43 3	28 4	74	58 2	61	**
4. Subsample II, controlled for foreign-policy attitudes	19 1	53 2	27.7	47	46 8	69	**
5. Subsample III, controlled for education, political attitudes, and foreign-policy attitudes	33 3	48 9	17.7	45	54 4	52	*
6. Subsample IV, controlled for social status, group memberships, and foreign-policy attitudes	20 7	45 3	34.0	53	49 0	.74	**

[a] Yule's Q, calculated on proportions of samples above and below median category

[b] χ^2 computed on basis of 50/50 assumption, based on proportions of samples above and below median category; $df = 1$.

* $P < 01$

** $P < 001$.

politically active, including activity in such organizations as peace groups.[10]

Thus far, we have considered only membership or nonmembership in peace groups. It was noted earlier that the peace groups varied in degree of radicalism, with the AAUN group being most conventional, the UWF group next, the CPG being more radical, and the radical pacifists the most radical. If the various scales are predictive of variations in radicalism of behavior, then for any given scale the several samples should have the rank order: (1) nonmember sample, (2) AAUN members, (3) UWF members, (4) CPG members, and (5) radical pacifist sample. Table 5 summarizes the data on the seven relevant scales.

With minor exceptions, my expectations are borne out on all scales except that indexing meaninglessness. The political attitudes scale and the foreign-policy attitudes scale are the most predictive of differences between peace workers and nonmembers, but are not of much value for predicting degrees of radicalness among peace activists. On the political attitudes scale, there is only a 23% difference between the lowest and highest of the four peace-activist categories.[11] While AAUN members are clearly separated off by the foreign-policy attitudes scale, there are only very small differences among the other groups.

In the case of the four alienation measures, some variations from the earlier picture emerge. The more radical the form of peace activity, the more alienated are the participants in normlessness and isolation—just as we would have predicted from the earlier gross comparison. However, in normlessness and isolation, only the two most radical categories of peace workers are sharply differentiated from the nonmember sample. In fact, on normlessness, AAUN members are actually less alienated than nonmembers. But the greatest variation from earlier expectation is in the case of meaninglessness. Here, the more alienated the peace activist, the more radical his activity, but the members of all three moderate peace groups are less alienated in the meaninglessness sense than are the nonmembers, while radical pacifists are markedly more alienated in this regard than are either moderate peace-group members or nonmembers. The greater degree of meaninglessness among radical pacifists than among nonmembers may,

[10] In all the samples, political liberalism and a sociological mode of explanation of social events were positively associated. Since many political conservatives are politically active, even though probably holding to a biopsychological form of explanation of social events, it is apparent that other factors also enter into political activism. However, for the total sample, the association between the social explanation scale and the political activity scale was significant at the .001 level ($\chi^2 = 26.39$)

[11] The rather surprising lower mean step of the radical pacifists than of the CPG probably reflects the fact that the scale offered only relatively conventional political questions and alternative answers. Some radical pacifists rejected the most liberal alternatives offered because they were too conventional. A scale extending to a really revolutionary pole would almost certainly have found the radical pacifists at an extreme position.

TABLE 5

COMPARISON OF NONMEMBER SAMPLE, MEMBERS OF THREE PEACE GROUPS, AND RADICAL PACIFISTS ON SEVEN SCALES

Scale	Nonmember Sample (N = 234)	AAUN[a] (N = 29)	UWF[a] (N = 21)	CPG[a] (N = 45)	Radical Pacifists (N = 30)	Ranking Errors
	Samples Ranked by Degree of Radicalness of Activity					
Political attitudes scale:						
a) % in most liberal third of scale ..	13.6	69.0	66.7	88.9	90.0	1
b) Mean step (of 9 steps)	5.3	6.8	7.2	8.1	7.6	1
Foreign-policy attitudes scale:						
a) % in most internationalist third of scale	4.7	34.5	81.0	82.2	93.3	0
b) Mean step (of 9 steps)	4.8	6.6	7.2	7.4	8.0	0
Powerlessness scale:						
a) % in most alienated 40% of scale	67.9	37.9	19.0	11.1	3.3	0
b) % in least alienated 40% of scale	18.1	31.0	57.1	80.0	90.0	0
c) Mean step (of 10 steps)	6.1	4.8	3.7	3.4	3.1	0
Meaninglessness scale:						
a) % in most alienated 40% of scale	41.8	13.8	19.0	31.1	63.3	1
b) Mean step (of 10 steps)	6.1	4.1	4.5	5.6	6.9	1
Normlessness scale:						
a) % in most alienated 44% of scale	17.2	6.9	9.5	62.2	83.3	1
b) Mean step (of 9 steps)	3.4	2.5	4.1	5.8	6.8	1
Isolation scale:						
a) % in most alienated 30% of scale	7.3	10.3	23.8	62.2	70.0	0
b) Mean step (of 10 steps)	4.1	4.5	5.9	7.5	7.7	0
Social explanation scale:						
a) % in sociological half of scale ...	34.0	72.4	85.7	95.6	100.0	0
b) % in two highest sociological categories of scale	12.5	31.0	61.9	73.3	86.7	0
c) Mean step (of 9 steps)	3.8	5.4	6.4	7.1	7.5	0

[a] In cases where an individual belonged to two peace groups, he is classified in the group in which he was most active. Cases belonging to two groups in which they were equally active are classified in the more radical group.

of course, simply reflect the fact that the nonmember sample was drawn from a high-status community in which there is relatively little alienation. At any rate, among the four peace-activist samples, we now find that normlessness, isolation, and meaninglessness all increase with degree of radical activity, while powerlessness decreases as radicalism increases.

The social explanation scale is highly predictive of degree of radicalism, providing a perfect ranking and sharp differentiation among the five ranks. There is, then, again confirmation that this scale may index an important orientation underlying degree of radical political activity. In summary, table 5 suggests that alienation is of little importance as a predisposing factor for those who enter moderate types of peace action (AAUN and UWF, while political and foreign-policy attitudes and form of social explanation are crucial factors. But for the more radical types of peace action (CPG and radical pacifists), alienation may operate as an important predisposing factor, along with political and foreign-policy attitudes and mode of social explanation. An *absence* of feelings of powerlessness is important for both moderate and radical peace action.

Tables 6 and 7 approach the problem in yet another way: are the seven scales indexing social-psychological orientations predictive of different *degrees* of peace activity? Respondents from the three community peace groups were asked to rate their involvement in their group's activities as "active," "sporadically active," or "member but not involved in activities." No consistent variations were found between the last two categories, and they are combined for this analysis. In table 6, the radical pacifist sample is again treated as the extreme rank. These persons either worked full time in peace activities or had these activities as the core of their identity and spent very large amounts of time in these activities. No one in the three moderate groups was a full-time peace worker. It is clear that for the radical pacifists, peace activities represent not only a more radical but a much more complete involvement than for the members of the three moderate groups.

In addition to the nonmember sample, table 6 includes as its second rank the subsample of nonmembers which, for each scale, had the greatest similarity (that is, the lowest Q) to the peace-group member sample. The subsamples included constitute the portion of the total nonmember sample most like the peace-group sample on a range of relevant characteristics but who had not engaged in organized peace activity. Thus, the subsample column represents the zero line for degree of peace activity among respondents whose social and political characteristics presumably make them "eligible" for peace activities.

Table 7 is also concerned with the relation of the seven scales to level of activity in peace work. Here, the active and less active members of the

TABLE 6

COMPARISON OF SEVEN SCALES OF NONMEMBER SAMPLE, NONMEMBER SUBSAMPLE MOST SIMILAR TO PEACE-GROUP SAMPLE, AND MEMBERS OF PEACE-ACTIVIST SAMPLES BY DEGREE OF ACTIVITY

Scale	Samples Ranked by Degree of Activity in Peace Work: Nonmember Sample ($N = 234$)	Nonmember Subsample ($N = 45–74$)[a]	Less Active Members ($N = 46$)	Active Group Members ($N = 49$)	Radical Pacifists ($N = 30$)	Ranking Errors
Political attitude scale:						
a) % in most liberal third of scale ..	13.6	41.9	74.4	83.7	90.0	0
b) Mean step (of 9 steps)	5.3	6.8	7.4	7.6	7.6	0
Foreign-policy attitudes scale:						
a) % in most internationalist third of scale	4.7	21 2	61.0	77.5	93.3	0
b) Mean step (of 9 steps)	4.8	5.7	6 8	7 4	8.0	0
Powerlessness scale.						
a) % in most alienated 40% of scale	67.9	42 2	23.9	18.4	3.3	0
b) % in least alienated 40% of scale	18.1	37.8	58.7	63.3	90 0	0
c) Mean step (of 10 steps)	6 1	5.3	3 9	3.9	3 1	0
Meaninglessness scale:						
a) % in most alienated 40% of scale	41.8	31.1	28 3	18.3	63 3	1
b) Mean step (of 10 steps)	6 1	5.9	5 1	4.7	6.9	1
Normlessness scale.						
a) % in most alienated 44% of scale	17.2	17.8	32 6	34.7	83.3	0
b) Mean step (of 9 steps)	3.4	3 3	4 4	4.4	6.8	1
Isolation scale:						
a) % in most alienated 30% of scale	7.3	22.7	43 0	32.6	70 0	1
b) Mean step (of 10 steps)	4.1	4.7	6.4	6 1	7.7	1
Social explanation scale:						
a) % in sociological half of scale	34 0	54 4	80.4	91.8	100 0	0
b) % in two highest sociological categories of scale	12.5	33.3	50 0	65.3	86 7	0
c) Mean step (of 9 steps)	3.8	4 8	6.1	6 7	7.5	0

[a] Number of cases varies because different subsamples were used in different comparisons using criterion of taking a subsample which showed the least difference from the total peace-group sample

TABLE 7

COMPARISON OF ACTIVE AND LESS ACTIVE MEMBERS OF THREE PEACE GROUPS ON SEVEN SCALES, BY MEAN STEP

SCALE	AAUN Less Active Members (N = 12)	AAUN Active Members (N = 17)	UWF Less Active Members (N = 6)	UWF Active Members (N = 15)	CPG Less Active Members (N = 28)	CPG Active Members (N = 17)	ERRORS FROM PREDICTED DIRECTION	MEAN DIFFERENCE IN PREDICTED DIRECTION
Political attitudes scale	6 7	6 9	6 0	7 7	8 0	8 2	0	0 7
Foreign-policy attitudes scale	3 6	6 2	6 3	7 6	6 6	7 7	0	1 7
Powerlessness scale	5 4	4 4	3 8	3 7	3 2	3 7	1	0 2
Meaninglessness scale	4 0	4 1	5 0	4 3	5 6	5 6	1	0 2
Normlessness scale	2 3	2 6	3 8	4 2	5 4	6 4	0	0 6
Isolation scale	4 2	4 6	4 7	6 4	7 6	7 3	1	0 6
Social explanation scale	5 1	5 7	5 2	6 9	6 8	7 5	0	1 0

three community peace groups are compared in terms of mean step on each scale.

The data in tables 6 and 7 give further confirmation of the hypothesis that variations on the social-psychological scales are systematically associated with behavioral differences. The political attitudes scale and the foreign-policy attitudes scale both differentiate samples perfectly for level of activity, though the political attitudes scale is not very useful for discriminating among peace workers. Among the types of alienation, powerlessness has a negative relation to degree of activity. However, rather surprisingly, a sense of power is a much weaker predictor of level of activity than of radicalism. Except for the radical pacifists, normlessness and isolation are not of value as predictors of level of activity, though they differentiate members and nonmembers more satisfactorily than for radicalism. Tables 6 and 7 offer some clarification of the role of meaninglessness. Among persons with moderate peace views, meaninglessness has no relation to level of activity, while a high degree of meaninglessness is associated with intense involvement in radical peace activities. In table 5, I found that degree of radicalness increases directly with extent of meaninglessness Thus, it appears that a sense of meaninglessness is associated with a high level of activity only where—doubtless in combination with other factors —it inspires a high degree of radicalness.

In both tables 6 and 7, the social explanation scale differentiates sharply and consistently between levels of activity: the more the sample accepts a sociological mode of explanation, the higher the level of activity.

We may now look at the interrelations of the various measures. The radical pacifists are not markedly different from the much more moderate CPG and UWF members on political attitudes or foreign-policy attitudes Nor are the radical pacifists greatly different from the CPG members in sense of powerlessness or mode of explaining social events. But radical pacifists are unique in being highly alienated in three senses: meaninglessness, normlessness, and isolation. Since degree of radicalness among moderate peace-group members is also directly related to degree of alienation in all three of these senses, *it seems reasonable to conclude that a high degree of meaninglessness, normlessness, and isolation, in combination with a high sense of being able to influence a social environment perceived in terms of a sociological mode of causation, predisposes persons toward radical social action.*

DISCUSSION

The basic theoretical problem of this paper is the relation of social-psychological orientations to social behavior. I have shown that a number of orientations are systematically associated with complex *patterns* of social

action. In tables 1–7, there are 192 comparisons of samples, and only 10 deviations from the initially predicted directions. Three of these deviations are connected with meaninglessness, which analysis showed is subject to interpretation making these apparent deviations nonerrors. Another of the 10 errors was between segments of the nonmember sample, leaving us with only 3% errors in the prediction of membership, radicalness, and level of activity from seven kinds of social-psychological orientations.

The analysis also shows that it is configurations of social-psychological orientations rather than single orientations which are associated with different kinds of action. However, which configuration is crucial seems to vary with the problem. For example, if our problem is simply predicting peace-group *membership*, a combination of political and foreign-policy attitudes explains the bulk of the variance: less than 5% of the nonmember sample overlaps with the more than 65% of the peace-group members who scored in the three most extreme steps on *both* nine-step scales. But if the question has to do with *radicalness* of peace activity or *level* of activity, then political attitudes or foreign-policy attitudes, or a combination of these, do not account for much of the variance. For these questions, the alienation scales and the mode of social explanation scale (the latter being the most consistent and discriminating predictor) become more crucial.

However, in relating orientations to action, a difficulty arises from the fact that an indefinite number of other orientations besides those considered here could be adduced which also discriminate significantly between kinds and levels of activity. In the present study, religious orientation, political preference, altruism, rationality, and attitudes toward communism all differentiated peace-group members and nonmembers significantly in all comparisons, including controlled subsamples (though for a large number of scales and items, no significant differences were found). While techniques such as factor analysis can cluster orientations, such techniques cannot determine whether the data-gathering procedure has included all relevant orientations.

Another difficulty in interpreting findings about the relation of orientations and action lies in the question of whether the social-psychological orientations cause or result from group involvements. In the absence of before-and-after data, no conclusive answer can be given in the present study. However, considerable relevant data were collected, and they suggest a complex answer. Interviews on recruitment were conducted with 25 active members as well as leaders of the peace groups. Interview and questionnaire data point to two conclusions: (1) recruitment into peace groups is less often the result of self-selection of the group by the recruit than of being recruited through belonging to social networks, some of whose members already belong to the peace group; and (2) the orientations predisposing individuals to peace activities are not, at least in their immediate

forms, internalized in childhood but develop in youth and early adulthood and certainly, for the bulk of peace activists, prior to membership in a peace group.

A high proportion of peace activists had a history of membership in liberal organizations or "causes," though only a small portion had previously been members of peace groups. Most recruits into the CPG and UWF were already associated with persons who belonged to or were organizing the peace group, and were recruited through these interpersonal channels. Even when recruitment was not directly interpersonal, recognition from newspaper stories of persons known in previous groups was a crucial factor in joining peace groups. Three chains of group affiliation were identifiable. One led from a cooperative parents nursery organized some years earlier, through a Unitarian Fellowship subsequently formed by many of the same people, and then into peace activity. A second chain began with an anti-capital-punishment group formed two years earlier, through a race-relations group then organized by most of the same people, and thence to the CPG. The third chain involved several groups associated with activities of the Friends, activities which involved some people who were not Quakers. In addition, many peace-group members had been associated with one another in a local Democratic Club or through a chapter of the American Civil Liberties Union. Informal social networks were also important in recruitment to the AAUN chapter, which was instigated largely by one person. Most of the very deliberate recruitment process was carried on by a small core of women already associated in the informal social network of the founder.

Selective recruitment is also reflected in the fact that the active core of each peace group was derived from distinguishable status groups in the community. The AAUN (whose members were to a considerable degree sought to provide a certain image) recruited largely from the highest-status sectors of the community and, in general, from middle-of-the-road political liberals. Membership included high-level university administrators and tenured faculty and was unique among peace groups in having a significant number of persons from the business sector. The UWF, though the most heterogeneous as the community's oldest peace group, drew most heavily from the more affluent (but subadministrative) segments of the university and from other prestige professions, such as law, medicine, and the ministry, but almost not at all from the business world. The UWF drew mainly from political progressives as contrasted with the more cautious liberals of the AAUN. The CPG recruited overwhelmingly from university-connected people; only one-sixth of its members were not affiliated with the university. However, these people were largely younger, nontenured members and, to a much higher degree than the other two groups, nonfaculty, such as research associates, librarians, or graduate students.

Thus, it is clear that recruitment is not a simple matter of predisposing social-psychological orientations but is much influenced by social relations and status. But it would be equally wrong to conclude that these orientations are of no significance. Most persons in the various status groups mentioned did not join a peace group, even when invited. The AAUN's initial recruitment list had two-thirds of its prospects from outside the academic community, but in fact, the group could get only about one-third of its members from the outside. Similarly, only a minority of such groups as the Unitarian Fellowship and the Democratic Club even became involved in a peace group. In many cases, ideological orientations were crucial in refusing membership, even in social networks of friends.[12]

The history of high participation in liberal activities and "causes" prior to peace-group membership indicates that most members had congenial social-psychological orientations before their recruitment into a peace group. (Only 20% of the moderate peace-group members were under 35 years of age, though two-thirds of the radical pacifists were under 35.) However, the evidence suggests that these orientations were not, for most members, simply internalized in childhood. Very few peace-group members had been raised in traditionally pacifist religions. Even in the radical pacifist sample, 83% had not been raised as pacifists. The evidence points to the hypothesis that the social-psychological configuration crucial to eventual peace activity came in youth and early adulthood. Most suggestive was the extent to which peace activists had changed religion since childhood. Religion was categorized as "none," "Roman Catholic," "Jewish," "fundamentalist Protestant," "middle Protestant" (e.g., Methodist, Presbyterian, Episcopalian), "liberal Protestant" (e.g., Unitarian, Quaker, Congregational), and "other." Taking mother's religion as the index of childhood religion, only 31% of the nonmember sample had changed in religious category, whereas 62% of moderate peace-group members, 67% of the "active" members of these groups, and 77% of the radical pacifists had changed. The direction of change among nonmembers was random (that is, as many in the fundamentalist direction as toward "liberal" or "none"), while virtually all changes by peace-group members and radical pacifists were toward more liberal religious preferences or no religion. All evidence indicates that such changes come most often in youth.

Another piece of evidence suggesting development of congenial orientations in youth and early adulthood is the large proportion of peace activists

[12] This interpretation was offered by core members of the peace groups who were interviewed with regard to recruitment into the groups. In addition, the author, as a member of the university faculty and of a number of community groups, had a rather close acquaintance with a number of refusers and found the ideological interpretation consistent with his own knowledge of these cases. There were enough instances of people espousing other unpopular causes but rejecting peace-group membership to justify discounting facile explanations such as fear of loss of social esteem

who had majored in the social sciences in college. Among those who had attended college, only 14% of nonmembers but 42% of moderate peace-group members and 56% of radical pacifists had majored in a social science. These data are especially significant in conjunction with the finding of the importance of a sociological mode of explanation of social events. The chains of group affiliation—such as that from a cooperative parents nursery, to the forming of a Unitarian Fellowship (few members of which had previously been Unitarians), to peace activities—are also suggestive of young-adult socialization into the kinds of views leading to peace activity. The fellowship grew out of a desire of these young parents for intellectual and ethical discussion.

If sufficient information were available about the chain of group interactions which preceded joining a peace group, it seems likely we could show how patterns of social relationships in youth and early adulthood had generated the social-psychological orientations congenial to peace activity for persons who were ultimately to join peace groups. But if a preceding sequence of group interactions molded these orientations, we must also suppose that participation in a peace group further shapes the orientations of members. That is, if we reject the notion that adult life is simply an acting out of social-psychological orientations internalized during childhood, it is no more logical to suppose that orientations developed between, say, 18 and 30 unalterably control behavior in middle age. It is probable, at least for individuals intensely involved in a peace group, that personal orientations are sharpened or modified by the experience. The intense activities of radical pacifists are not merely an acting out of alienation but an expression of identification with their particular groups. That these groups (such as CNVA and WRL) are sectarian associations in conflict with society implies that they intensify feelings of alienation in their members. The extreme alienation of the radical pacifists is thus probably partially a reflection of selective recruitment and partially a matter of intensification of alienation by participation in radical groups. Among the three community peace groups, the most intense involvement was unquestionably among active members of the CPG. It is a reasonable hypothesis that the relatively more extreme position of CPG members on most scales reflects, in part, an accentuation of those orientations which helped lead them into peace work in the first place.

Thus, no simple answer can be given to the question of whether social-psychological orientations are cause or effect of action in groups. The individual is not an actor but an interactor. To separate out social-psychological orientations from the individual's group involvements is an artifice of our techniques. By the same token, there is no simple way to answer the question of whether the kinds of orientations I have dealt with are basic personality qualities or simply transitory aberrations. These orientations

surely transcend particular situations, but they seem subject to alteration in the course of group involvement. What is called for is that very difficult kind of longitudinal study in which changes in personal orientations are related to changes in group contexts over a number of years.

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Parents, Peers, and Delinquent Action: A Test of the Differential Association Perspective[1]

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This study attempts to go beyond the well-documented relationship between delinquent associations and involvement in delinquency to a consideration of the independent consequences of delinquent peers, parents, and "delinquent" definitions for delinquent action. The data fail to support Sutherland and Cressey's argument that family life is relevant to delinquency only when "delinquent patterns" are available to copy. Using a variety of measures of availability of deviant patterns, paternal supervision and support were found to be negatively related to delinquency to approximately the same degree under almost all conditions. Moreover, delinquent peers and paternal supervision and support were both found to influence delinquency involvement regardless of definitions favorable and unfavorable to the violation of the law. The family, peers, and definitions relevant to law breaking appear to exert independent effects on delinquency which are not adequately encompassed by etiological perspectives that introduce such definitions as intervening between other important variables and delinquency.

INTRODUCTION

Most empirical tests of the differential association perspective on delinquency (Sutherland and Cressey 1966, pp. 77–100) have focused on the most basic relationship implied by the theory: association with delinquent peers is assumed to lead to differential exposure to "definitions favorable to the violation of the law" and is subsequently examined in relation to delinquent behavior.[2] A strong positive relationship has been well documented (Glueck and Glueck 1950, pp. 163–64; Short 1957, pp. 233–39; Voss 1964, pp. 74–85; Erickson and Empey 1965, pp. 268–82). The present study represents an attempt to go beyond the usual concern with the direct relationship between differential association and delinquency to

[1] The data utilized in this paper were collected as part of the Richmond Youth Study under the direction of Alan B. Wilson (Survey Research Center, University of California, Berkeley). I gratefully acknowledge my debt to Travis Hirschi. This paper elaborates, in part, on some aspects of his earlier analysis of the Richmond data as reported in *Causes of Delinquency* (Berkeley: University of California Press, 1969)

[2] Two notable exceptions are Hirschi's *Causes of Delinquency* (1969) and Hackler's study of "norms vs. others" as predictors of delinquency (1968, pp. 92–106).

an assessment of the consequences of peers, parents, and definitions for delinquency involvement and an empirical examination of the ability of the theory to encompass a certain set of "known" relationships between patterns of family life and delinquency.

DIFFERENTIAL ASSOCIATION AND THE FAMILY

An adequate theory of delinquency must be able to encompass the persistent finding that the nature of parent-child relationships is a fairly important determinant of involvement in delinquency (Healy and Bronner 1936; Glueck and Glueck 1950; Nye 1958; Gold 1963; Hirschi 1969), and the differential association perspective has been posited as just such a theory.[3] Recognizing such persistent relationships, Sutherland and Cressey (1966, pp. 226–27) argue that most family variables are related to delinquency through the following two processes:

> A child may be driven from the home by unpleasant experiences and situations or withdraw from it because of the absence of pleasant experiences, and thus cease to be a functioning member of an integrated group. . . . The important element is that isolation from the family is likely to increase the child's associations with delinquency behavior patterns and decrease his associations with antidelinquency behavior patterns.
>
> The home may fail to train the child to deal with community situations in a law-abiding manner. That is, delinquency patterns may not be present in the home, but the home may be neutral with respect to delinquency of the child. . . . Again, whether such a "neutral" child becomes delinquent or not will depend upon his associations with delinquent and antidelinquent patterns outside the home

The first of these two processes encompasses situations of isolation or alienation from the home as distinct from the failure of the home to present antidelinquent patterns. In the latter situation, the child is not necessarily driven or withdrawn from the family, but the family situation is such that he fails to develop attitudes, inhibitions, or definitions which work against the violation of the law.

In either case, whether the "unattached" child becomes delinquent depends on his associations *outside the home* (Sutherland and Cressey 1966, pp. 227–28).

[3] Since the hypotheses to be tested in this analysis treat conditions of family life as independent variables and delinquency as the dependent variable, our analysis and interpretations are in terms of that assumed causal ordering. However, it should be recognized that delinquency may be causally implicated in generating certain conditions of family life as well. Similarly, when we test certain notions derived from the differential association perspective, the interpretation is in terms of the ordering suggested by such theorists. Again, it should be recognized that, even where the associations suggest that certain relationships do exist, the causal direction may be quite different from that assumed by a particular theorist.

> These two processes are important because they increase the probability that a child will come into intimate contact with delinquents and will be attracted by delinquent behavior.
>
> A child does not necessarily become delinquent because he is unhappy. Children in unhappy homes may take on delinquency patterns if there are any around for them to acquire.

While the stress is placed on contacts outside the home, Sutherland and Cressey do note that a child may learn to be delinquent in the home as well. They feel, however, that peers of the same sex are more important than parents in shaping the behavior of adolescents, whether in delinquent or antidelinquent directions. Hence, their emphasis is on contact with children of the same age and sex.[4]

In sum, while numerous theorists and researchers have supported a theoretical model locating the development of delinquent behavior in the nature of direct and indirect control by the family over the teenager, Sutherland and Cressey's arguments add a qualifying condition to the model. The lack of control by parents is argued to be associated with delinquent behavior only in situations where there are delinquent patterns around to copy. In short, the known relationships between qualities of family life and delinquency are thought to hold up only within certain contexts.

Robert Stanfield (1966, pp. 411–17) has found several patterns of "interaction" consistent with such an hypothesis. For example, using appearances in a court for one or more juvenile offenses as his measure of delinquency, he found parental discipline to be more strongly related to delinquency among low-status families than high-status families, and he interprets that finding as suggesting that "unpleasant family experiences are more likely to produce delinquency in circumstances where there is an alternative cultural pattern that is favorable to the violation of the law." However, recent research (Hirschi 1969, pp. 212–23) suggests little, if any, correlation between family status and acceptance of alternative cultural patterns favorable to the violation of the law. While such findings cast doubt on the notion that it is such "cultural" patterns which account for the conditioning effect of social status, it may be that status is related to association with delinquents and that such associations increase the probability of delinquency through group pressure rather than through socialization into competing cultural standards alone. At any rate, Stanfield's

[4] Other authors have argued that a "Fagin" pattern of socialization is rare (Sykes and Matza 1957, p. 665); and, in support of such contentions, Hirschi (1969, pp. 94–97) has found that, irrespective of social class standing, attachments to one's parents are negatively related to delinquency. He notes that "the lower-class parent even if he is himself committing criminal acts, does not publicize this fact to his children . . . He operates to foster obedience to a system of norms to which he himself may not conform."

study is a suggestive indirect test of the "interaction hypothesis," based on a small, homogeneous, and nonrandom sample.

This study attempts a more direct and encompassing test through an examination of the relationship between direct and indirect parental control and delinquency under conditions varying in the availability of delinquent patterns, utilizing data from a large heterogeneous random sample of adolescents.[5] Moreover, since differential association theory has put so much emphasis on "definitions," we test some rather basic but as yet unexamined claims of the theory involving such normative phenomena. Do delinquent peers influence acceptance of unconventional cultural standards, and, if so, do delinquent peers affect delinquency only through such alternative cultural patterns? One of the major implications of Short and Strodtbeck's (1965) group-process perspective on delinquency is that delinquent peers can influence involvement in delinquency independently of definitions favorable and unfavorable to the violation of the law. While such definitions are likely to be related to delinquent behavior and to be, in part, a product of interaction with delinquent friends, it can be hypothesized that the delinquent peer group may be a source of "situationally induced motives" (Briar and Piliavan 1965, pp. 35–45) and that delinquent peers can thus provide the impetus to deviate before one has come to accept unconventional definitions and quite often in spite of commitments to conventional normative standards.

STUDY DESIGN

These relationships will be examined utilizing questionnaire data gathered as part of the Richmond Youth Study by the Survey Research Center (University of California at Berkeley) in 1965. The population consisted of the 17,500 students entering the 11 junior and senior high schools of Western Contra Costa County in the fall of 1964. While the original sample consisted of both black and nonblack, male and female adolescents in grades 7 through 12, the present analysis was carried out on the 1,588 nonblack males in the sample.[6]

[5] One of the many findings in Hirschi's (1969, pp. 154–55) earlier multivariate analysis of this body of data was that attachment to parents is negatively related to delinquency, regardless of number of delinquent friends an adolescent has. In the present analysis, we elaborate on that finding through more extensive controls for exposure to criminogenic influences as well as "definitions" favorable to law breaking. Moreover, while Hirschi focused on the independent role of such attachments in the etiology of delinquency, the present analysis focuses more extensively on the differential association theorists' prediction of interaction effects.

[6] The sample was drawn from the student population of Western Contra Costa County, California, which is located in the San Francisco-Oakland metropolitan area, bordered by Berkeley and the San Francisco and San Pablo Bays. The largest city in the area is Richmond, which is primarily an industrial community, with more than

The dependent variable, delinquent behavior, was measured by the respondent's answers to a series of six questions concerning offenses of varying degrees of seriousness.[7] Only acts committed within a year previous to the administration of the questionnaire are included in the score. The respondents are categorized as admitting to no delinquent acts, one delinquent act, and two or more delinquent acts.

Supervision and emotional support by the father were measured by means of questionnaire items. Paternal supervision was assessed by asking respondents to answer "usually," "sometimes," or "never" to the following questions: (1) Does your father know where you are when you are away from home? and (2) Does your father know who you are with when you are away from home? Paternal support was measured by answers to the three questions: (1) Does your father seem to understand you? (2) Have you ever felt unwanted by your father? (3) Would your father stick by you if you really got into bad trouble?

Number of close delinquent friends, perceptions of "trouble" in the neighborhood, and official delinquency rates of the schools attended are each used as indicators of variation in the presence of delinquent patterns. Respondents were asked, "Have any of your close friends ever been picked up by the police?" and were categorized for purposes of analysis into those with zero, one or two, and three or more delinquent friends. The adolescents in the sample were also presented the statement "Young people are always getting into trouble" and were asked whether or not it applied to their neighborhood. These two items are similar to measures of differential association used in other studies (Short 1957, pp. 335–36). Utilizing police data gathered from the Richmond and San Pablo police departments and the Contra Costa County Sheriff's Office, the schools sampled were ranked in terms of the percentage of nonblack "official delinquents" and classified as "high," "medium," and "low." There was not sufficient variation in self-reported delinquency from school to school to rank them in terms of self-reported delinquency.

As DeFleur and Quinney (1966, p. 7) specify in their formalization of

60% of the employed males holding manual jobs. For more extensive details on the area, see Wilson (1966). The nonblack segment of the sample consisted primarily of Caucasians (90%), with a small proportion of Orientals and "others." For a more detailed discussion of sampling and data-gathering procedures, see Wilson, Hirschi, and Elder (1965) and Hirschi (1969).

[7] Self-reported delinquency was measured by responses to the following questions (1) have you ever taken little things (worth less than $2.00) that did not belong to you? (2) have you ever taken things of some value (between $2.00 and $50.00) that did not belong to you? (3) have you ever taken things of large value (worth over $50.00) that did not belong to you? (4) have you ever taken a car for a ride without the owner's permission? (5) have you ever banged up on purpose something that did not belong to you? (6) not counting fights you may have had with a brother or sister, have you ever beaten up on anyone on purpose?

the differential association theory, it is criminal motives, attitudes, and techniques which are assumed to effectively result in the overt commission of crimes. Associating with delinquents is thought to be conducive to learning definitions favorable to the violation of the law, and an excess of such definitions over antidelinquent definitions is assumed to lead to delinquency. Sykes and Matza (1957, pp. 664–70) suggest that "techniques of neutralization" are a crucial component of these definitions, in that a definition favorable to the violation of legal codes can be viewed in one sense as a justification of deviant behavior. Two of the items making up the "definitions" score are implicated in a discussion of these techniques: (1) most things that people call "delinquency" don't really hurt anyone, and (2) policemen try to give all kids an even break. The first item is concerned with the "denial of injury," while the second is assumed to be involved in the "condemnation of the condemners." Two other items went into the score: (1) it's alright to get around the law if you can get away with it, and (2) to get ahead, you have to do some things which are not right. On all but the police item, "strongly agree" or "agree" was coded zero, "undecided, 1; and "disagree" or "strongly disagree," 2. The codes were reversed for the police item. These four items were combined to form a score that measures criminal attitudes or definitions favorable to the violation of the law. No measure of the respondent's knowledge of techniques was available, limiting this study to a score measuring only the attitudinal content of the learning that leads to delinquency.

Gamma, a symmetrical measure of association with a "proportional reduction in error" interpretation appropriate to an ordinal level of measurement (Goodman and Kruskal 1954, pp. 732–64; Costner 1965, pp 341–53) and percentage distributions are used to assess the association between variables. The differential association variables are controlled by subdivision, and the relationships of paternal supervision and support to delinquent behavior are examined within these subcategories.

FINDINGS

The zero-order relationships among paternal supervision and support, number of delinquent friends, perception of neighborhood trouble, and delinquent behavior are summarized in table 1. All of the gamma coefficients are statistically significant at the .01 level. As expected, both paternal supervision and support are negatively related to delinquency. Moreover, the data are consistent with Sutherland and Cressey's argument that the nature of a child's home life can affect the probability that he will come into intimate contact with delinquent peers, in that paternal supervision and support are negatively related to intimate associations with others who have been picked up by the police. They are similarly related

TABLE 1

ZERO-ORDER GAMMA COEFFICIENTS

Variables	Delinquent Friends	Delinquent Definitions	Neighborhood Trouble	Delinquent Behavior
Paternal supervision	−.37	−.25	−.18	−.28
Paternal support	−.28	−.29	−.23	−.30
Delinquent friends	...	.43	.26	.60
Delinquent definitions	...	..	.18	38
Neighborhood trouble	...	...	...	.25

to "delinquent" attitudes and beliefs and to perceptions of trouble in the neighborhood. However, as the strength of the relationships suggests, many well-supervised and emotionally supported adolescents have delinquent friends, engage in delinquent action, and exhibit tenuous commitments to conventional moral standards.

The three differential association variables are, in turn, related to involvement in delinquent behavior. The more delinquent associates one has, the more likely he is to be delinquent. Similarly, the less the attachment to the conventional normative system and the greater the perception of trouble in the neighborhood, the greater the tendency toward delinquent action. Not only are all three related to the overall measure of delinquency, but each was found to be associated with every act that went into the overall measure as well with all the gamma coefficients in the expected direction and significant at the .01 level. Moreover, number of delinquent friends, neighborhood trouble, and the definitions score are interrelated as well with an especially strong relationship exhibited between intimate associations with delinquents and "delinquent" attitudes and beliefs. Associating with persons assumed to embrace attitudes and beliefs favorable to the violation of the law is, as expected, positively associated with embracement of such attitudes and beliefs.

However, as predicted on the basis of group-process and situational-inducement perspectives on delinquency, association with delinquents and the definitions score are independently related to delinquency involvement (table 2). Together, these two variables differentiate adolescents in terms of delinquency fairly well. While only 22% of the adolescents with no delinquent friends and a preponderance of definitions unfavorable to the violation of the law have committed one or more delinquent acts, fully 80% of the adolescents at the other end of the continuum fall in that category. However, it is clear that the effect of the number of delinquent friends on delinquency is not solely a product of socialization into competing normative standards. The partials are somewhat smaller than the zero-order coefficient of .60 but are still quite strong relative to other rela-

TABLE 2

SELF-REPORTED DELINQUENCY WITHIN "DEFINITIONS" SUBGROUPS, BY DELINQUENT FRIENDS

	DEFINITIONS FAVORABLE AND UNFAVORABLE TO THE VIOLATION OF THE LAW								
	Unfavorable (%)			Neutral (%)			Favorable (%)		
SELF-REPORTED DELINQUENT ACT	0	1–2	3+	0	1–2	3+	0	1–2	3+
0	78	64	38	69	49	34	60	37	20
1	18	26	24	19	33	33	24	33	29
2+	4	10	38	11	18	34	15	30	51
N	251	81	34	202	105	98	91	86	138
Gamma		.47			.43			.49	

tionships in the data. As suggested by Briar and Piliavan (1965), it may be that adolescents with delinquent friends are more likely to experience short-run, situationally induced pressures to deviate.

To assess Sutherland and Cressey's (1966) hypothesis, it is necessary to go beyond the zero-order relationships to an examination of more complex three-variable relationships. For example, tables 3 and 4 summarize the relationships between paternal supervision and support and delinquency involvement, controlling for number of delinquent friends. It turns out that, while parental supervision is negatively related to the number of close delinquent friends and number of delinquent friends is positively associated with delinquency, the relationship between supervision and delinquent behavior persists within all three categories of delinquent friends. The data fail to support Sutherland and Cressey's (1966) argument, in that the association is equally strong among those with three or

TABLE 3

SELF-REPORTED DELINQUENCY WITHIN "DELINQUENT FRIENDS" SUBGROUPS, BY PATERNAL SUPERVISION

	NUMBER OF DELINQUENT FRIENDS								
	3+ (%)			1–2 (%)			0 (%)		
SELF-REPORTED DELINQUENT ACTS	Low	Medium	High	Low	Medium	High	Low	Medium	High
0	26	28	30	39	50	59	60	78	74
1	21	37	35	36	28	29	28	17	18
2+	53	35	36	26	22	13	13	5	8
N	119	57	98	90	60	111	120	96	346
Gamma		—.17			—.26			—.17	

TABLE 4

SELF-REPORTED DELINQUENCY WITHIN "DELINQUENT FRIENDS" SUBGROUPS, BY PATERNAL EMOTIONAL SUPPORT

	NUMBER OF DELINQUENT FRIENDS											
	3+ (%)				1–2 (%)				0 (%)			
SELF-REPORTED DELINQUENT ACTS	Low	2	3	High	Low	2	3	High	Low	2	3	High
0	21	20	33	43	44	52	49	56	61	68	71	80
1	24	39	33	26	30	30	36	32	21	22	21	17
2+	55	41	33	31	26	18	15	12	18	10	8	3
N	104	59	51	49	86	54	47	72	104	100	140	190
Gamma		−.27				−.15				−.28		

more delinquent friends and among those with no delinquent friends at all. Thus, paternal supervision appears to be related to delinquency, irrespective of the availability of delinquent patterns as measured by the number of close delinquent friends. Similarly, supportive or affectionate father-son relationships are negatively related to delinquent behavior within each of the delinquent-friends subdivisions, with each variable contributing independently to delinquent behavior.

By subdividing on the basis of perceptions of trouble in the neighborhood, it was possible to test the hypothesis that "if the family is in a community in which there is no pattern of theft, the children do not steal, no matter how much neglected or how unhappy they may be at home" (Sutherland and Cressey 1966, p. 227). The data indicate that paternal supervision is negatively related to delinquency even when respondents disagree that the young are always getting in trouble in their neighborhood (see table 5). In fact, supervision is actually slightly less strongly related

TABLE 5

SELF-REPORTED DELINQUENCY WITHIN NEIGHBORHOOD CONTEXTS, BY PATERNAL SUPERVISION

	YOUNG ALWAYS GETTING IN TROUBLE								
	Agree (%)			Undecided (%)			Disagree (%)		
SELF-REPORTED DELINQUENT ACTS	Low	Medium	High	Low	Medium	High	Low	Medium	High
0	27	70	39	46	52	63	52	57	69
1	29	9	28	25	27	23	24	30	22
2+	44	21	32	29	21	14	24	13	9
N	90	43	71	72	62	124	243	169	468
Gamma		−.20			−.25			−.28	

to delinquency in neighborhoods where the young are perceived as always in trouble than among those where they are perceived as little trouble.

Neither does paternal support appear to interact significantly with perceived trouble (table 6). Emotional support does lead to slightly greater percentage differences among respondents in neighborhoods high in trouble. However, the relations persist within all subdivisions, and the gamma coefficients are only slightly different. As was the case with number of delinquent friends, when perception of trouble in the neighborhood is used to measure the availability of delinquent behavior patterns, the data fail to reveal the predicted pattern of interaction.

Table 7 summarizes the relationships between paternal supervision and support and self-reported delinquency in schools with variable delinquency rates; and, again, there appears to be no tendency for direct and indirect paternal control to make any greater difference in high-rate schools than in low-rate schools. For example, the gamma coefficients combining all low-rate schools (7.6%–10.8% with an official record) were —.26 for the paternal support measure and —.30 for paternal supervision as compared with —.32 and —.27 for all high-rate schools combined (26.2%–32.7% officially delinquent). Again, we fail to find support for the interaction hypothesis. Whether in a school with a high rate or a low rate of official delinquency, paternal supervision and support differentiate adolescents in terms of admitted delinquent acts equally well.

We have already seen that the probability of embracing "definitions favorable to the violation of the law" increases with increases in the number of close friends who have been picked up by the police. These definitions are, in turn, associated with delinquency involvement. Moreover, each of these variables, independent of the other, is related to delinquency. When the "definitions" score is held constant by subdivision,

TABLE 6

SELF-REPORTED DELINQUENCY WITHIN NEIGHBORHOOD CONTEXTS, BY PATERNAL EMOTIONAL SUPPORT

Self-reported Delinquent Acts	Young in Neighborhood Always in Trouble: Agree (%)				Undecided (%)				Disagree (%)			
	Low	2	3	High	Low	2	3	High	Low	2	3	High
0	34	40	40	67	47	47	60	72	49	57	68	69
1	20	20	34	23	22	34	27	21	27	28	21	22
2+	46	40	26	10	31	22	12	7	24	15	11	9
N	83	45	35	31	81	65	48	57	212	157	204	278
Gamma		—.32				—.30				—.25		

TABLE 7

Gamma Coefficients relating Self-reported Delinquency to Paternal Supervision and Support within Schools

Schools and Percentage Officially Delinquent	Self-Report by Supervision	Self-Report by Support
High rate:		
33%	−.30 ($N = 92$)	−.27 ($N = 93$)
29%	.03 ($N = 31$)	−.19 ($N = 29$)
26%	−.29 ($N = 215$)	−.30 ($N = 212$)
26%	−.17 ($N = 151$)	−.29 ($N = 144$)
Middle rate.		
21%	−.44 ($N = 102$)	−.34 ($N = 100$)
21%	−.41 ($N = 112$)	−.44 ($N = 100$)
19%	−.10 ($N = 29$)	−.11 ($N = 26$)
Low rate:		
11%	−.32 ($N = 285$)	−.15 ($N = 267$)
11%	−.28 ($N = 143$)	−.39 ($N = 140$)
8%	−.27 ($N = 184$)	−.38 ($N = 177$)

we find that paternal supervision and support are related to delinquent behavior in all subgroups as well (see tables 8 and 9). The relationships are slightly stronger among those with attitudes and beliefs conducive to law breaking than among those committed to more conventional standards, in that father's supervision and support are slightly more likely to have an effect among those who express attitudes and beliefs conducive to the violation of the law. In short, parental surveillance and support may make for a somewhat greater difference in delinquency involvement among adolescents

TABLE 8

Self-reported Delinquency within "Definitions" Subgroups, by Paternal Supervision

Self-reported Delinquent Acts	Definitions Favorable to Violation								
	Favorable (%)			Neutral (%)			Unfavorable (%)		
	Low	Medium	High	Low	Medium	High	Low	Medium	High
0	26	44	45	50	54	64	68	75	78
1	29	36	28	29	30	24	21	16	17
2+	46	20	27	21	17	12	12	8	5
N	145	59	115	127	115	229	96	73	245
Gamma		−.29			−.20			−.18	

TABLE 9

SELF-REPORTED DELINQUENCY WITHIN "DEFINITIONS" SUBGROUPS, BY PATERNAL EMOTIONAL SUPPORT

Self-reported Delinquent Acts	Definitions Favorable to Violation: Favorable (%)				Neutral (%)				Unfavorable (%)			
	Low	2	3	High	Low	2	3	High	Low	2	3	High
0	26	39	46	50	52	46	63	70	70	69	74	79
1	30	29	31	30	27	32	26	23	16	22	17	18
2+	44	32	23	20	21	21	11	7	14	9	9	4
N	131	75	52	54	132	99	101	121	70	68	104	159
Gamma		—.29				—.25				—.16		

who are not committed to conventional normative standards and who hold attitudes favorable to the violation of the law. Among adolescents committed to conventional standards, such external parental control may have lesser impact because of the marked strength of internal controls resulting from a firm acceptance of conventional definitions.[8] This interaction should not mask the fact that, contrary to Sutherland and Cressey's (1966) arguments, parental supervision and support are related to delinquency even in the categories exhibiting definitions unfavorable to the violation of the law.

SUMMARY AND CONCLUSIONS

This study attempts to go beyond the usual concern with the relationships between delinquent companions and delinquent behavior to an assessment of arguments using differential association theory to encompass certain "known" relationships. Consistent with previous research and as predicated on the basis of the theory, the number of delinquent friends, the perception of "trouble" in the neighborhood, and the variable acceptance of attitudes and beliefs favorable to the violation of legal codes were significantly related to involvement in delinquent action. Moreover, those associating with delinquents are more likely to be delinquent, regardless of the effect of these associations on their attitudes and beliefs. This finding tends to support theories of delinquency which stress the importance of group

[8] This finding is consistent with the claims of "containment theorists" (e.g., Reckless 1966, pp. 223–30; 1967) that elements of "inner containment" may play a greater role as deterrents to delinquency in situations of weak "outer containment" than in situations where external controls are more likely to be operative. For a critical analysis and reformulation of the containment perspective, see Jensen (1970, pp 1–14; 1972).

pressure, group processes, and short-term situational motivations in the explanation of delinquency.

Using several alternative measures of the availability of delinquent patterns, the findings consistently tended to cast doubt on Sutherland and Cressey's (1966) arguments concerning the home and family in relation to delinquent behavior. Number of delinquent friends had little effect on the relationship between paternal supervision and delinquency. Nor did it have much of an effect on the association between affectionate father-son relationships and delinquent behavior. The case was similar when neighborhood patterns and school-delinquency rates were held constant. Some interaction was noted when definitions or verbalizations favorable to the violation of the law were controlled: the two family variables were more highly related to delinquent behavior among those whose definitions were most favorable to the violation of the law. However, the relationships persisted within all subdivisions.

In sum, what goes on in the family situation appears to have a significance of its own which is not encompassed by the differential association perspective as presented by Sutherland and Cressey (1966). The neutral or isolated child is more likely to be delinquent than the child who is loved by and attached to his parents even when delinquent patterns "outside the home" are scarce or absent. Moreover, contrary to some studies (Watt and Maher 1958, pp. 321–30), these data suggest that attitudes toward one's parents do affect attitudes toward public law and morality but that direct and indirect parental control have effects on delinquent behavior independent of such beliefs and attitudes. In short, the family has effects on delinquency involvement which cannot be attributed to the "internalization" of values, beliefs, or attitudes towards the law. Such findings are inconsistent with theories that view attitudes and beliefs concerning legal prescriptions as "the" variables accounting for the consequences of family life for delinquency.

As Marvin Olsen (1968, pp. 117–29) points out, actors are led to perpetuate patterns of social organization, not merely because they come to accept certain social norms as their own, but also through processes of social sanctioning, social manipulation, identification, and compliance. Just as actual and potential reactions of delinquent peers can influence behavior without necessarily shaping normative commitments at the same time, so the sensitivity of children to the actual and potential reactions of their parents may shape their behavior even if they do not form commitments to such standards. The theory of differential association stresses "definitions" and "cultural" variables (values, norms, and beliefs) to such an extent that processes shaping human behavior other than internalization of normative standards tend to be slighted.

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Accepting "Significant Others": Six Models[1]

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Six alternative models of the ways in which individuals accept and organize information from potential sources are proposed. Parameter estimates are obtained from a set of experiments ($N = 110$), and the models are tested against data from an independent set of experiments ($N = 114$). Results of the tests favor a simple additive model. Two of the models proposed are elaborations of ideas recently proposed by Berger and Fisek (1970), and results of our tests are generally consistent with theirs. Further applications of the models to experimental and to natural settings are drawn, and some consequences of the results of the tests are discussed.

SELF-CONCEPT AND INFORMATION THEORY

Self-concept, according to the tradition growing from the writings of Cooley (1902) and of Mead (1934), is dependent upon the attitudes and opinions of others. If we restrict attention to evaluative aspects of the self, then this tradition would assert that the individual's high or low self-evaluation is a function of the positive and negative evaluations he receives from others. Moreover, the self-evaluation is dependent upon the evaluations received from a particular kind of others, "significant others," in Sullivan's (1947) terminology. In this paper, we propose and test alternative models intended to describe the processes by which the self incorporates others' evaluations.

The proposed models are tested in a series of laboratory experiments designed to evaluate propositions on the development of conceptions of ability of self and others. One experiment tested the proposition that a crucial determinant of becoming a significant other—that is, one whose

[1] In addition to the authors, experiments were conducted by John B. Kervin and David Grafstein, and we wish to acknowledge their contributions to the work. Doris R. Entwisle generously provided much helpful advice in planning the project. I Richard Savage offered detailed, constructive criticism of the paper Funding was provided by the following: NSF grant GS 2169; U.S. Office of Education grant OEG-2-7-061610-2; and by Stanford University and Johns Hopkins University We gratefully acknowledge this financial assistance.

evaluations "mattered" to the individual—is the individual's perception of other's ability at the task. An evaluator perceived to have high ability to perform the task himself was predicted to be accepted as a significant other. If the evaluator were perceived to have low ability, the individual was predicted to be far less likely to "cognize" the evaluations and to incorporate them into his ability conception. These predictions were confirmed (Webster 1969).

However, an interesting result of this first experiment is that the opinions of a low-ability evaluator were not totally ignored. They were relatively unlikely to affect the cognitions and future actions of the individual, but at least in some cases they did produce a measurable effect. This suggests the desirability of attempting to develop a more complete understanding of the ways in which individuals incorporate information from their environments in general and evaluations of ability from others in particular. The perspective we adopt is one shared by a family of theories which are loosely grouped together as "information theory."

From an information theoretic approach, the individual is viewed as an information processing system; he perceives, interprets, and assimilates data, and his cognitions and future actions are assumed to be systematically related to the nature of the information data. But it is clear that not all input sensory data are equally useful as information. The individual must select, evaluate, and combine data to reach a useful conclusion. The mechanisms of this process thus become of central importance.

For studying the information selection and utilization process, we formulate two specific questions. (1) How does the individual utilize conflicting information from various sources? and (2) how does the individual utilize congruent information from two differentiated sources, or from two equivalent sources? As answers, we propose six possible models of information processing. The models are assessed using data from two series of similar experiments, conducted at different times, but with comparable subject pools and nearly identical experimental procedures. The purpose and the design of the experiments have been reported in other works (Sobieszek 1972; Webster 1969); only aspects relevant to the models and the model testing we perform here will be described in this work.

The experiment consists of two parts, or phases. In phase 1, pairs of subjects enter the laboratory and perform a task which requires them to make 20 binary judgments about a series of slides. Each judgment is communicated to a third individual, who has been described as possessing either unusually high ability to make these judgments or unusually low ability. This third individual, the evaluator, supposedly decides whether each subject's judgment is correct and communicates this information to him Actually, the situation is controlled in a number of ways. First, the slides have no objectively correct answers, and in fact extensive pretesting

has established that in the absence of social influence, the likelihood of choosing either is very close to .50 (see Webster 1969 for details). Thus it is plausible to the subjects that the evaluator's choices might disagree with or agree with their own. Second, communication between individuals is completely controlled. During the experiment, subjects cannot see each other, each other's choices, or the evaluator. (The evaluator is described as being another student from the same school who is seated in another room, but he exists only as a tape-recorded voice.) Thus it is possible for the evaluator to give either positive or negative evaluations to one or both subjects, regardless of their actual choices to a given slide, and the effects of any cues about the evaluator (other than his ability) are minimized and are standardized across conditions.

At the end of the 20 slides in phase 1, the evaluator announces his opinion of the subjects' performances. To each subject he gives either a high proportion of positive evaluations or a high proportion of negative evaluations. At this point, if a subject believes the evaluator, he should think that he is either very good at the task, or that he is very poor Thus there are two possible values for the evaluator's ability (high or low), and two possible types of evaluations given to a subject (much better than his partner or much worse). Four conditions are produced by varying the evaluator's ability (the H or L conditions) and whether he gives the subject highly positive evaluations (the [+] conditions) or highly negative evaluations (the [—] conditions).

The data from the experiment are gathered in phase 2, when a second series of slides is presented. For each slide, each subject makes an initial choice which is communicated to his partner, restudies the slide, and then makes a private final choice. Communication is again controlled, so that subjects are told that their initial choices are in virtually continual disagreement. The basic operational prediction of the theory is that the higher the subject's conception of his own ability relative to that of his partner, the less likely he is to accept influence when he is told that their initial choices conflict. The proportion of times that subjects in each condition resolve disagreements in favor of self, $P(s)$, is the main statistic used to test these predictions. For example, a subject who refuses to change any of his initial choices would have a $P(s) = 1.00$; a subject who changes 16 out of the 20 disagreeing initial choices would have a $P(s) = .20$.

Combining the notation for the evaluator's ability and for the nature of his evaluations, the theory predicts the following ordering of conditions by the $P(s)$ statistic: H[+] > L[+] > L[—] > H[—].[2] Results from

[2] The explicit derivation of this prediction is contained in the earlier report (Webster 1969). Exact theoretical reasons for the predictions are not central to this report and

TABLE 1

P(s) VALUES FOR THE SINGLE-SOURCE EXPERIMENT

Condition	*N*	*P(s)*
1. H[+]	19	.79
2 L[+]	19	.64
3. L[−]	20	.56
4. H[−]	18	.46

80 subjects in this experiment—which we will refer to as the "single-source" experiment—are given in table 1.[8]

In terms of information processing, three facts are significant. First, the observed ordering of conditions indicates that the individual has accepted and incorporated the evaluation information in phase 1 of the experiment and has used it to determine a feature of his behavior in phase 2 Second, in those conditions of the experiment where the only information available is from an individual described as possessing unusually low ability (the L conditions), subjects do not totally disregard the evaluations: the *P(s)* for the L[+] condition is greater than the *P(s)* for the L[−] condition. Third, not only is the relative evaluation information utilized (the *P(s)* for respective [+] conditions is greater than the *P(s)* for [−] conditions), but also, the source of the information is utilized; the effect of either positive or negative evaluations from the H is greater than the effect of the same evaluations from the L.

In order to use these results to describe precisely the ways in which subjects utilize information from two evaluators—both when that information is consistent and when the two evaluators disagree with each other—we need to estimate the strength of effect of each of these factors. In addition, we need a baseline estimator from which we can assess the additional positive or negative effects of the ability of the evaluator and of the [+] or [−] evaluations.

Such an estimator is the *P(s)* value produced in a second experiment in which the phase 1 evaluations were never communicated to subjects.

so are omitted; however it should be clear from the predicted ordering that *P(s)* is expected to vary directly with two factors· (1) the H or L ability of the evaluator, and (2) the [+] or [−] evaluations given to the subject

[8] Data from about 5%–10% of subjects in these experiments were excluded from analysis on grounds of considerable evidence that subjects did not meet one or more conditions of the experimental design. For example, subjects who clearly did not believe that the disagreements were real could not be said to be accepting or rejecting influence in making their final choices, and so were excluded. Complete criteria for exclusion as well as details on how the inclusion/exclusion decisions are made are available in Webster 1970.

They were told that their performances were being evaluated in phase 1, but that they would not be told the evaluations until after the entire study was completed. All other details of phases 1 and 2 were identical with the experiment previously described. This experiment may be called the "no-source" experiment, or O-S; the $P(s)$ in the O-S was .62 ($N = 30$).

Taking the .62 probability from the O-S as the estimate of the baseline effect of the disagreements without any evaluations, this figure may be subtracted algebraically from each condition of the single-source experiment in order to determine a table of evaluation weights, shown in table 2

TABLE 2
ESTIMATED EVALUATION WEIGHTS FOR THE SINGLE-SOURCE EXPERIMENT

Evaluation	Weight
H+	+.17
H−	−.16
L+	+.02
L−	−.06

These weights are an estimate of the combined effect of the evaluator and the evaluation in each of our H[+], H[−], L[+], and L[−] conditions.

By using the baseline estimator and our evaluation weights, we can predict the $P(s)$ values from other experiments in which there are two phase 1 evaluators.[4] We can predict the $P(s)$ values for situations in

[4] Because we make direct comparisons of data across these various experiments, a word about comparability of the situations and of the subject pools is in order. The experiments are all variants of a basic experimental situation developed at Stanford by Joseph Berger and his associates. Differences between experiments are slight, being limited to the changes required for tests of the different versions of the theory. The differences probably would not be noticeable to an observer who was not closely acquainted with the particular theory derivations under test. Subjects for the experiments reported were volunteers recruited from English courses at a California junior college. At the time of the experiments, none was under 17 years of age, and none was over 24. The single-source experiments were conducted in spring 1968 with male subjects. The two-source experiments were conducted in spring 1969 with female subjects, except for the O-S and H[+]H[−] conditions, which were conducted in summer 1970 with female subjects. All subjects were recruited from regular classes at the college, not from evening or summer sessions. Though the time difference may have produced slight differences in the subject pools, in terms of any of the frequently used measures—such as academic ability, racial or ethnic background, SES of families, etc.—there was no change during the time these experiments were conducted. The sex difference between the single-source experiments (male subjects) and the later experiments (female subjects) conceivably could make direct comparisons of data dangerous, but in the absence of specific information regarding the effect of sex in these experiments, we have chosen to treat this difference as inconsequential.

TABLE 3

$P(s)$ VALUES FOR THE TWO-SOURCE EXPERIMENT

Condition	N	$P(s)$
1. H[+]L[+]	21	.80
2. H[+]L[−]	20	.75
3 H[+]H[−]	27	.67
4 H[−]L[+]	20	.57
5. H[−]L[−]	21	.42

which these two evaluators possess either equal or unequal ability and where they either agree or disagree as to the nature of their evaluations of performance.[5]

The two additional experiments for which the models may make predictions both involve subjects in phase 1 receiving evaluations from two evaluators. In the first, one evaluator in every group is described as possessing unusually high ability and the other as possessing unusually low ability. These two evaluators either agree (conditions 1 and 5 in table 3) or disagree (conditions 2 and 4) on their evaluations of each subject's performances. In the second experiment, shown as condition 3, both evaluators are described as possessing unusually high task ability, and they disagree on their evaluations of each subject's ability. The observed $P(s)$ values from these five conditions are shown in table 3.[6]

[5] There are 20 critical (disagreement) trials in all these experiments For all tables in this report, the $P(s)$ was calculated for the last 15 disagreement trials only, since there is reason to believe that the first few trials of the disagreement phase are used by subjects to adjust to an unfamiliar situation Both variance across subjects and alternating behavior are greater for the first block of five trials than for any subsequent block of trials Also, there is no theoretical reason to expect a systematic change in results as a function of the disagreement resolution process, nor is there evidence of such a change in these experiments Data published by Moore (1969), as well as examination of unpublished data from 12 conditions reported by Camilleri and Berger (1967), show that for most conditions which use this experimental situation, the $P(s)$ curve drops (about .02–.09) between the first and second quarter of critical trials and usually is relatively flat thereafter. This is the case for most of our conditions reported in tables 1 and 3 here, and it seems to be a general result produced by the disagreement trials We know of no theoretical reason to expect the drop, but it seems plausible to us that a subject would not fully consider a partner's disagreeing initial choice until he had time to practice a bit with the equipment and with the decision-making task Thus we conclude that the final three blocks of trials provide the most representative and stable estimate of $P(s)$ for each condition. If $P(s)$ data were calculated on the basis of all 20 trials, the figures for most conditions would be the same or higher (though no more than .02 in any case). This would change some individual condition comparisons in tables 4–8 and 10–11, but it would not alter relative assessments of the models

[6] Note in table 2 that $|H-| \simeq |H+|$ and $|L-| > |L+|$. The self-evaluation theory which guided these experiments does not give any reason to expect that under some circumstances negative evaluations would produce greater effect than positive evalua-

THE MODELS AND THE TESTING

Prediction of results of these more complex experiments depends upon the way in which individuals are believed to utilize information. This process may be represented by a variety of models, derived from a corresponding variety of different substantive theories.[7] We will consider six models which seem to represent intuitively plausible assumptions and which make distinguishable predictions for behavior in this experimental situation. Then we will assess each model, based both upon the empirical

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tions from the same evaluator, nor is any feature of this general experimental design clearly related to the observed effect. Therefore it might seem reasonable to impose the added condition that effects of positive and negative evaluations from the same evaluator have the same degree of effect, or more formally: $|H+| = |H-|$ and $|L+| = |L-|$. This restriction offers the advantage of increasing the amount of information used to estimate the evaluation effect parameters, for two conditions then would be used for each. As the simplest way of using two conditions, the effects of the positive and negative evaluations from the same source could be averaged, to obtain the following values: evaluating H(+ or −), weight $\pm$ .17; evaluation L(+ or −), weight $\pm$ .04. However there is no readily apparent justification for adding such a restriction to parameters, and use of these values would not change the results of any comparisons to be made later in this paper. Therefore, we have decided to use the simplest estimates of the evaluation effect parameters, those shown in table 2, and to note that they were estimated on the basis of minimal information (usually, about 20 subjects for 15 trials, or 300 units of information).

[7] Many investigators, from learning theorists to methodologists, have reported work on issues related to information combining, defined broadly (see Bush and Estes [1959]; Coombs, Dawes, and Tversky [1970]; and Rosenberg [1968*a*] for recent summaries). However much of this work is not directly applicable to our interests here Questions of handling data are probably least closely related. Coombs (1964, pp. 284–91) discusses the need to find single measures from incomparable components, for example, a single IQ score from the underlying factors measured by the test items This is an objective combining procedure—presumably the result of a deliberate decision made after explicitly considering various weighting options. By contrast, our models are designed to reflect subjective processes by which individuals—not necessarily deliberately—reach a single measure such as ability self-evaluation or overall status of an individual from either consistent or inconsistent units of information How individuals select and combine units of information to reach a single opinion or attitude has been studied most often by cognitive psychologists. The issue whether additive models (similar to our model 3) or averaging models (similar to our model 4) are preferable has been debated (see Feldman 1968, pp. 744–55; and Kepka and Brickman 1971), though apparently without a clear-cut preference emerging. For situations of interest to psychologists—such as forming an overall impression of someone based upon knowledge of several adjectives purported to characterize him—the issue may well be highly relevant, but for more sociological concerns (such as selecting significant others or evaluating ability of others from knowledge of status characteristics) the number of information units is probably smaller, and what seems more relevant is whether some information is completely ignored (our models 1 and 2). In any case, we feel that Rosenberg's criticism (1968*b*, p. 765) is well taken; namely that this research relies heavily upon giving subjects scant information about a hypothetical other in a necessarily underdefined experimental setting, and then asking him to report his opinions of the other or how he might treat him. We hope our work constitutes a contribution since our results are based upon observing actual, not hypothetical, behavior which is predicted by the models.

consideration of success of prediction and upon theoretical considerations of simplicity and adequacy.

The six models fall into two general categories: additive models and operator models. Additive models assume that the combining process may be adequately represented by a summation of available information—the individual adds units of information, which may or may not be equal in importance, to arrive at the total information which forms the basis for his action. Operator models assume that the individual takes the unit of information and performs some more complicated operation upon it before assimilating the information. In this experimental situation, for example, one simple additive model (3) assumes that the individual adds up the evaluations from two sources to come to a total weight which he assigns to the information. A simple operator model (6) assumes that there is some baseline effect, such as the $P(s)$ of the O-S experiment, which is then proportionally altered by the effect of evaluations and the ability of the evaluator. Therefore, estimates derived from an operator model are a function of the baseline probability, while estimates derived from additive models are independent of the baseline probability. Since additive models assume the more simple combining process, we will examine them first.

A typical term of the additive models is shown by $P_O + E_{H+} + E_{L+} = P_{H+L+}$, where $P_O = P(s)$ for the O-S experiment, E_{H+} = the estimated evaluation weight of a positive evaluation from the high-ability evaluator, E_{L+} = the estimated evaluation weight of a positive evaluation from the low ability evaluator, and $P_{H+L+} = P(s)$ for the H[+]L[+] condition of the two-source experiments.

1 The Single-Source Model

When the individual is aware that more than one other individual is giving him opinions, a first step in utilizing this information is to decide whether to accept all available data, or to perceive it selectively and to ignore some of it. In this experimental situation, the question is whether a subject who is receiving evaluations from two evaluators will pay attention to both of them or whether he will simplify the situation cognitively by choosing only one of them. The first model we examine assumes the latter process.[8] Thus, it becomes necessary to specify which evaluator

[8] This model is implicitly adopted by many of those who have worked in the areas of cognitive consistency and distortion The assumption is frequently made, for example, that potential information which conflicts with (or is dissonant with) the individual's previous beliefs (or with his psychological needs) will not be accepted so readily as information which is consistent (see Heider 1946, 1958; Secord and Backman 1961, for similar statements). Though our results do not provide a definitive test for acceptance or rejection of these orientations, it is important to point out that the theoretical issues being examined have implications beyond the specific models

will be accepted and which ignored. A basis for making this specification is provided in the original theory which guided these experiments. When both a high-ability evaluator and a low-ability evaluator are available, it seems reasonable to expect that the individual will accept the high-ability evaluator and will ignore the low-ability evaluator. For these experiments, this process would have two consequences: First, the $P(s)$ from the H[+] condition would equal the $P(s)$ from the H[+]L[+] and the H[+] L[—] conditions. Second, the $P(s)$ from the H[—] condition would equal the $P(s)$ from the H[—]L[+] and the H[—]L[—] conditions.

For the case of two disagreeing high ability evaluators (the H[+] H[—] experiment), it is still difficult to predict which of them would be accepted. To obtain a simple estimate of $P(s)$, we will assume that half the subjects in this experiment accept each evaluator. Those who accept only the H+ evaluations will display the same $P(s)$ as subjects who received positive evaluations from one high-ability evaluator in the single-source experiments; those who accept only the H— evaluations will display the $P(s)$ of subjects in the comparable condition of the single-source experiments. The respective $P(s)$ figures for these two conditions are .79 and .46, which we average to obtain an estimate of .63 for the H[+]H[—] experiment from this model. The $P(s)$ figures predicted by this model are shown in table 4, with the figure for the H[+]H[—] experiments in brackets to indicate that an additional assumption was required to get the predicted figure. The general additive model was used for calculating the predicted figures, with the restriction for the two-evaluator situation that $E_{L+} = E_{L-} = 0$. Therefore, $P_0 + E_{H+} = P_{H+L}$.

TABLE 4

SINGLE-SOURCE MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

Condition	Expected $P(s)$	Observed $P(s)$	Discrepancy (|Exp — Obs|)
1 H[+]L[+]	.79	.80	.01
2 H[+]L[—]	79	.75	.04
3. H[+]H[—]	[.63]	67	[.04]
4 H[—]L[+]	.46	57	.11
5. H[—]L[—]	46	42	04

Recent experimental work reported by Berger and Fisek (1970) has addressed similar issues. Their results appear to support the conclusion that the individual will accept all available information and will somehow combine it, rather than accepting consistent information and rejecting inconsistent information Therefore, support of the first model from our data would be inconsistent with Berger and Fisek's conclusion; rejection of this model would constitute an independent confirmation of their conclusion.

Comparison of predicted with observed figures in table 4 indicates that this first model provides a close prediction of the $P(s)$ for condition 1 and moderate success for conditions 2, 3, and 5. The approximation of prediction to observed results is less close for condition 4.

2 The Single-Source Given Disagreement Model

A variant of the first model assumes that the tendency of the individual is to accept more than one source of information, so long as all sources are consistent. When potential sources disagree in their information, then the individual will choose one of them and disregard the other, as in the first model. More precisely, this second model assumes that the individual will follow an additive process for consistent information (conditions 1 and 5 of the two source experiments). When the potential sources disagree (conditions 2, 3, and 4) he will resolve the situation by accepting only one source. In other words, this model asserts that the cognitive distortion implied in the idea of accepting only one of several potential sources will occur only when the sources conflict with each other. Table 5 presents the expected and observed $P(s)$ values for this second model.

TABLE 5

SINGLE-SOURCE GIVEN DISAGREEMENT MODEL EXPECTED AND OBSERVED $P(s)$ VALUES

	Condition	Expected $P(s)$	Observed $P(s)$	Discrepancy (|Exp − Obs|)
1	H[+]L[+]	81	80	01
2	H[+]L[−]	79	75	04
3	H[+]H[−]	[63]	67	[.04]
4	H[−]L[+]	46	57	.11
5	H[−]L[−]	40	42	.02

This elaborated model provides a good fit to the observed data for condition 1 and a better fit than the single-source model for condition 5. It makes the same predictions of conditions 2, 3, and 4. The single-source given disagreement model thus provides a better fit than the simple single-source model, the simplest of the balancing models.

3. The Simple Additive Model

The third model assumes that the individual accepts all information available in the situation and simply sums it—both when sources agree and when they disagree—in order to arrive at a useful conclusion. The formula for calculating predicted values for the simple additive model is the

TABLE 6

SIMPLE ADDITIVE MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

Condition	Expected $P(s)$	Observed $P(s)$	Discrepancy (|Exp. − Obs|)
1. H[+]L[+]	.81	.80	.01
2. H[+]L[−]	.73	75	.02
3. H[+]H[−]	.63	.67	.04
4. H[−]L[+]	48	.57	.09
5. H[−]L[−]	.40	42	.02

same as the general additive model, with no additional restrictions: $P_O + E_{H+} + E_{L+} = P_{H+L+}$. Table 6 reports the predicted and observed $P(s)$ values, using the simple additive model.

The simple additive model gives a better overall fit to the observed data than either the single-source model or the single-source given disagreement model. It retains the good predictions of conditions 1 and 5 in model 2, provides the same fit as models 1 and 2 for condition 3, and has a better fit for conditions 2 and 4 than either previous model. Also for condition 3, the predicted $P(s)$ and observed discrepancy are no longer bracketed, since both the H[+] and the H[−] are weighted in this model. In addition, we note the greater conceptual simplicity of the simple additive model than of the single-source given disagreement model Both on grounds of theoretical simplicity and on grounds of accuracy of empirical prediction, the simple additive model is to be favored over either of the two balancing models.[9]

4. The Averaging-Effects Model

The fourth model asserts that the total effect of each additional potential piece of information is diminished by the effect of every previous piece

[9] Both models 1 and 2 would predict two populations of subjects in all two-evaluator experiments: those who have accepted a source and those who have not This may be used to provide another type of test of these models. For condition 3 we assume that all (or nearly all) subjects will accept one and only one evaluator If this occurred, it would produce a bimodal distribution of subjects: those who accepted H[+] would have a high $P(s)$ and those who accepted H[−] would have a low $P(s)$ Examination of the data from condition 3 shows no evidence of bimodality, but rather a unimodal, approximately normal distribution about the mean. Also, existence of two distinct populations increases variance across subjects within a condition, but the variance across subjects of the O-S condition (the one most nearly comparable with the H[+]H[−] condition) is more than two times greater than variance within the H[+]H[−] condition. Thus the distribution of subjects in condition 3 is also inconsistent with models 1 and 2.

of information; that is, that information from more than one of several equivalent sources is averaged to produce the final cognition.[10] A typical term of an averaging model is: $P_{H+L+} = P_O + (E_{H+} + E_{L+})/k$, where k is the number of potential sources of information, and the other terms are interpreted as in the previous models.

This averaging model (or the elaboration of it proposed as model 5 below) appears to be an explicit statement of the combining model proposed by Berger and Fisek (1970). Thus, acceptance of a version of this model from our data would probably be consistent with their conclusion, and rejection would probably be inconsistent. However it should be noted that from their description of the combining model it is not possible to be certain that our averaging model adequately represents their ideas.

In any empirical operation, the averaging model seems most plausible for situations where the information available is inconsistent; that is, individuals may average inconsistent information but they will not do this for consistent information. To assert that individuals average information from one high-ability evaluator and one low-ability evaluator when the evaluators agree is to assert that the agreement from the low-ability evaluator in some way decreases the subject's confidence in the information from the high-ability evaluator. Thus, in our experimetal situation, this model predicts that the effect of a negative evaluation from a low-ability evaluator would decrease the strength of effect of a positive evaluation from a high-ability evaluator. Though at first it might seem intuitively unlikely, this assertion can plausibly be argued. The low-ability evaluator in these experiments is expected to make some mistakes in his evaluations, and if the high-ability evaluator always agrees with him, then the high-ability evaluator may be making a few mistakes also.

[10] This type of model seems to be of greatest interest to cognitive psychologists, and it possesses considerable intuitive appeal as well. Interestingly, Triandis (1968, pp. 723–30) concluded that for semantic differential measures of opinion and social distance measures of behavior toward hypothetical others, no important cultural differences exist to affect choice between a congruity model (similar to our 1), a summation model (similar to our 3) or a weighted average model (a more complex version of our 4) Triandis's data suggest that the summation model was slightly preferable cross-culturally on social distance but not on opinion items As we noted above, this result is based upon asking respondents how they would behave rather than by observing actual behavior For an individual confronted with large amounts of information—say knowledge of another's behavior in 20 different situations—this multiplying process seems intuitively likely; it says that all units of information will be utilized, but that the utility of each unit declines Anderson (1968, pp. 731–34) measured effects of various descriptive adjectives upon overall liking for a hypothetical individual. His results seem to favor a more complex version of our model 6. However, he also notes that Anderson and Jacobson (1965) concluded that inconsistent items are treated as less important than consistent items, a result which has some elements of our averaging (4) and operator (6) models.

Thus, agreement of a low-ability evaluator could be a factor in reducing the credibility of the high-ability evaluator: "If that stupid person agrees with him, then I don't believe what he says."

We first assume the simpler condition, that the averaging model may be applied to all cases. Table 7 presents a comparison of the observed $P(s)$ values and those which would be expected from a simple averaging model.

TABLE 7

SIMPLE AVERAGING MODEL· EXPECTED AND OBSERVED $P(s)$ VALUES

Condition	Expected $P(s)$	Observed $P(s)$	Discrepancy (|Exp. − Obs.|)
1. H[+]L[+]	72	80	08
2. H[+]L[−]	68	75	.07
3 H[+]H[−]	63	.67	.04
4 H[−]L[+]	55	.57	.02
5 H[−]L[−]	51	42	09

Compared to the simple additive model, the simple averaging model gives the same prediction for condition 3 and a better prediction for condition 4. However, the simple additive model gives better predictions for conditions 1, 2, and 5. Thus the simple averaging model is better than the simple additive model for only one case; however, that case is the one for which the simple additive model gives a particularly poor prediction.

5. The Averaging Given Disagreement Model

For model 5, we assume the intuitively more likely condition: that the individual averages information only when he is exposed to inconsistency, when the evaluators disagree. We make the further assumption that the simple additive model should be applied in cases of agreement, since it provided the best fit to those conditions. Table 8 presents the results of predicted and observed $P(s)$, using the model which assumes a simple additive process when information is consistent, and averaging when it is inconsistent.

The averaging given disagreement model retains the good prediction of condition 4 from the simple averaging model, and it incorporates the good predictions of conditions 1 and 5 from the simple additive model. All predictions from this model are reasonably close to the observed data except condition 2. By comparison with predictions from the simple additive model, however, predictions from the averaging given disagreement

TABLE 8

AVERAGING GIVEN DISAGREEMENT MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

	Condition	Expected $P(s)$	Observed $P(s)$	Discrepancy (|Exp. − Obs.|)
1	H[+]L[+]	.81	.80	.01
2	H[+]L[−]	.68	.75	.07
3	H[+]H[−]	.63	.67	.04
4	H[−]L[+]	.55	.57	.02
5	H[−]L[−]	40	.42	02

model are not significantly better. The simple additive model makes a considerably better prediction for condition 2, and the averaging given disagreement model makes a better prediction of condition 4. Both models make the same prediction for condition 3. Therefore, on the criterion of empirical prediction, choice between these models is not clear-cut.

On grounds of conceptual simplicity, the simple additive model is clearly preferable. The averaging given disagreement model requires that an additional assumption be made, and moreover, it provides no theoretical reason for using the simple additive model in cases where the available information units are consistent. For the tests shown in table 8, the simple additive model was used for the consistent evaluation conditions on the ad hoc grounds that it provided a good fit to observed data, but acceptance of the averaging given disagreement otherwise additive model would require a more complete justification of this. Such a justification, of course, remains to be worked out. Therefore, on grounds of theoretical simplicity, and especially in view of the fact that the simple additive model provides a reasonably good fit of the data, we conclude that it is to be preferred over the averaging given disagreement model.

6. A Simple Operator Model

An operator model assumes that an individual processes information through a multiplicative rather than an additive process. This model takes the O-S probability of rejecting influence (.62) as a baseline, and then multiplies it by an estimate of the multiplicative effect of positive or negative evaluations from a high-ability or a low-ability evaluator to obtain the observed $P(s)$ of the single-source conditions.[11] These estimates of multiplicative effect will be represented by α's.

[11] In general, additive models are conceptually simpler than operator models and therefore would be preferred so long as the operator model did not provide a markedly superior fit to the observed data. In view of the fact that the simple additive model has provided a close fit to the data, it does not seem likely at this point that predictions would be improved by an operator model. However at the time this

TABLE 9

α ESTIMATES FOR OPERATOR MODEL

Parameter	Estimate
α_{H+}	1 274
α_{H-}	0.742
α_{L+}	1.032
α_{L-}	0 903

In order to obtain the estimate of α for a positive evaluation from a high-ability evaluator, for example, we take the O-S $P(s)$ figure as the estimate of baseline effect of no evaluator and then determine the figure by which it must be multiplied in order to obtain the observed $P(s)$ for the H[+] condition. Thus the formula for obtaining α estimates for the H[+] conditions: $\alpha_{H+}(P_0) = P_{H+}$, or $\alpha_{H+} = P_{H+}/P_0$. Using this general formula and solving for the α estimates of the H[+], H[−], L[+], and L[−] conditions, we get the values shown in table 9.[12]

A typical term for the operator model is given by: $\alpha_{L+}[\alpha_{H+}(P_0)] = P_{H+L+}$, where P_0 is the $P(s)$ for the O-S experiment and P_{H+L+} is the predicted value for the H[+]L[+] experiment. Using this formula, the $P(s)$ values shown in table 10 would be predicted.

The operator model does not do a particularly satisfactory job of predicting the observed values for the two-evaluator experiments. Only for conditions 1 and 5 are predictions reasonably close to the observed figures; for conditions 3 and 4 the fit is poor; and for condition 2 it is moderate. By comparison with the additive models considered, the operator model does less well than any of them at predicting the observed figures. In particular, the fit by the operator model is considerably poorer than it was by the simple additive model, the one which generally made

investigation was undertaken this outcome was not apparent, and it is always possible to improve upon a prediction; hence one simple operator model was formulated for testing Especially in view of the prevalence of models of this type in learning psychology and cognitive psychology (see Rosenberg 1968*a*), it seems worthwhile to examine the results of applying the operator model to this situation

[12] These estimates are dependent upon the O-S probability; by comparison, the additive models use estimates which are independent of the O-S probability That is, one may speak of the effect of a positive evaluation from a high-ability evaluator as having a value of +.17 (shown in table 2), and this value could be added to whatever the baseline probability for no evaluator might be in another experimental situation. This is not the case for the operator model, for the α estimates speak of a proportional increase over the given baseline probability for the given situation. Thus, the α estimates are specific to the experimental situation and to the subject pool used One could not use the α estimates from one experimental situation in a different situation, or use α estimates from one subject pool in another subject pool.

TABLE 10

Operator Model: Expected and Observed $P(s)$ Values

	Condition	Expected $P(s)$	Observed $P(s)$	Discrepancy (|Exp. − Obs|)
1	H[+]L[+]	.82	80	.02
2	H[+]L[−]	71	75	04
3	H[+]H[−]	.58	67	.09
4	H[−]L[+]	47	57	.10
5	H[−]L[−]	.41	.42	01

the best predictions. Based upon these comparisons, we conclude that the additive models as a set, and especially the simple additive model, are preferable to operator models. Though operator models in general may have more intuitive appeal, the additive models are preferable on grounds both of conceptual simplicity and adequacy of empirical predictions.

DISCUSSION, THEORETICAL SIGNIFICANCE, IMPLICATIONS

The results of our model testing support the following conclusions. First, among the additive models, the simple additive model is preferable. It is the simplest model and the one which makes the most satisfactory predictions of observed data. Second, the additive models are preferable to the operator model for the same reasons; namely, the additive models are simpler, and they enable better predictions of the observed data.

In terms of the question of how individuals accept information from a variety of potential sources, the results of our comparisons suggest some more general conclusions. Whether individuals assimilate consistent information more easily than information which is *inconsistent*, or whether they accept all information without regard to consistency, has been studied repeatedly by different investigators in a variety of empirical settings. The former process is represented by our single-source and single-source given disagreement models; the latter, by the simple additive and the averaging models. The results of the comparisons of predicted and observed effects support the latter models and, thus, the latter process.

The recent experimental work by Berger and Fisek (1970) also has attempted to differentiate between these two processes (which they called, respectively, balancing and combining). Their results were partially consistent with ours; they found support for the combining process. However, Berger and Fisek report results which are restricted to cases in which individuals had conflicting information from two sources who were identical in plausibility, or in likelihood of being accepted: all information

came from an experimenter. In our experiments, the plausibility of the low-ability evaluator is lower than that of the high-ability evaluator. The differential plausibility permits making two sorts of discriminations between models which are not possible when all evaluative information comes from a source of high plausibility.

First, the differential permits distinguishing the results of combining information from the results of ignoring both potential sources, something which Berger and Fisek note is not possible in their experiments reported to date. For example, the $P(s)$ for our H[+]H[—] condition shown in table 3 is .67, which is close to the $P(s)$, figure of .62 for the O-S experiment. The figures from these two conditions would be expected to be nearly the same if subjects were either (1) accepting and combining information from both potential sources; or (2) ignoring both of them However, the fact that the $P(s)$ for the H[+]H[—] condition differs from both the $P(s)$ for the H[+] condition and from the H[+]L[—] condition is consistent only with the interpretation that subjects accept information from both potential sources and then combine it.

The second way in which our results differ from those of Berger and Fisek is that, whereas they compared only two general types of models (combining and balancing), we tested several specific versions of both types. In general, our results agree with theirs: the evidence for some sort of combining process was stronger than the evidence for a balancing process. However, the results are not strictly equivalent. The combining model which Berger and Fisek proposed and for which they found support seems most closely related to our simple averaging model (4), which asserts that individuals will combine bits of information by weighting them. While this model makes fairly good predictions to our observed data, our simple additive model (3) makes even better predictions. The simple additive model assumes that the individual combines information, not by weighting it, but simply by summing all consistent and contradictory information. For an experiment in which both sources of information are of equal importance, such as the experiment Berger and Fisek reported and our H[+]H[—] condition, it is impossible to distinguish the predictions of the simple additive model from the predictions of the simple averaging model. For this, data are needed from experiments in which the contradictory sources of information are not equally plausible, and our results for these conditions of our experiments were better for model 3. Thus it seems more accurate to consider our results a refinement of the theoretical ideas proposed by Berger and Fisek.

An especially appealing feature of the model which provided the best fit to the observed data, the simple additive model, is that it is the simplest of the six models proposed. The process of information utilization described by the simple additive model is even a simplification of the

process hypothesized by Berger and Fisek, if our interpretation of their ideas is accurate. We are encouraged that the processes of interpreting and assimilating information, which are often considered to be extremely complex, may be represented relatively simply for this situation. Of course, the results of similar experiments in more complex social situations—for example, with three evaluators—could well require a more complex theoretical conceptualization.

It may not be immediately apparent that the simple additive model represents one intuitively very plausible description of the way in which individuals utilize information; namely, that they give a positive or negative evaluation some weight, which is then multiplied by the individual making the observation to arrive at the overall importance of the information. Such a process may seem to be a multiplicative process at first glance, however it may easily be seen that it is not, by the following considerations. Using the intuitive model, we could say that the weight of a single evaluation in affecting the $P(s)$ is .01. Then the individual multiplies this weight by some factor determined by the source and by whether the evaluation was positive or negative, and finally the resulting figure is added to the baseline estimator to predict the $P(s)$ for any given combination of evaluators and positive or negative evaluations. At this point, it should be clear that the process described is no different from that envisioned in the simple additive model, for in fact what was multiplied by .01 then would be the evaluation weights shown in table 2. Though this description of the process of information usage is formally no different than the earlier description of the simple additive model, it may help to increase its intuitive appeal.

Finally, we may consider some implications of the numerical values used in this work for parameter estimates of effect of evaluations for the additive models. First, comparisons may be drawn between the estimates of effect of a positive evaluation versus a negative evaluation from the same evaluator by comparing the estimates of the H+ to H— and the estimates of L+ to L— in table 2. The estimates of effect of evaluations from a high-ability evaluator do not differ appreciably: positive evaluation is predicted to raise the $P(s)$ by .17; and negative evaluation, to lower the $P(s)$ by .16. However, the estimates of effect of evaluations from the low-ability evaluator do differ appreciably: positive evaluation is predicted to raise the $P(s)$ by only .02; and negative evaluation, to lower the $P(s)$ by .06. In absolute terms, the difference between .02 and .06 is small, and it is possible to ascribe it to chance factors operative in this experimental situation. But these estimates enable quite good predictions for the second series of experiments, in which there were evaluations from a low-ability evaluator. Moreover, though the absolute difference between .02 and .06 is small, the proportional difference is

considerable. Therefore, we may ascribe some reliability to these figures and consider an implication of this proportional difference.

The idea is frequently expressed in social psychology that individuals in some sense prefer a positive self-image, and thus that they will act, either behaviorally or cognitively, to maximize the actual or the perceived positive evaluations they receive from others. One way of maximizing positive evaluations from others is to emit behaviors valued by those others; for example, Zetterberg (1957) has postulated that individuals will give compliance in return for positive evaluations. A second way to maximize positive evaluations which has been frequently posited is cognitively to distort evaluations from others; either to change the valence of negative evaluations or to ignore a greater proportion of negative than of positive evaluations (for example, see Heider 1944; especially pp. 368–69). The behavioral means of seeking positive evaluations was not available to subjects in our experiments, but the cognitive means was available.

For the high-ability evaluator cases, it may not be surprising that the effects of the positive evaluations and the negative evaluations were virtually identical. The high-ability evaluator is one whose opinions would be difficult to ignore from any theoretical or intuitive viewpoint. But there is reason to believe that the low-ability evaluator may be overlooked by some subjects. Indeed, this is a central assertion of the theory under test in these experiments: the low-ability evaluator is perceived to be less competent to evaluate performance, and thus he is less likely to become a significant other whose evaluations become part of the self-evaluation of the individual.

In view of these considerations, it is striking that it is the positive evaluations from the low-ability evaluator which have the smaller effect in this situation. Whether his positive evaluations are ignored by a greater proportion of subjects, or whether all subjects ignore a greater proportion of his positive evaluations (an independent question which cannot be answered from the data we present here), it is the positive evaluations, not the negative evaluations, which are more ignored. If confidence may be placed in our estimated values, positive evaluations are ignored by a ratio of 3 to 1 over negative evaluations. This interpretation, of course, is directly counter to an asserted tendency to maximize the self-image, or at least it is counter to any asserted tendency consistent with the Cooley-Mead tradition regarding the sources of self-evaluation.

This conclusion may be seen more clearly by constructing and testing a model which assumes that the individual's tendency is to maximize the self-image through selective perception of the evaluations received from two evaluators.[13] Several variants of the basic model could be constructed;

[13] We thank an extremely conscientious and helpful (but anonymous) *AJS* reviewer

we shall examine only the simplest for illustration. In the simplest case, the subject is assumed to overlook negative evaluations, whether they come from the H or from the L evaluator. Thus, H[−] = L[−] = 0. So that we can use the estimated evaluation weights from table 2, we will assume that it is only in the more complex social situation of two evaluators that the negative evaluations are overlooked. In this model, H[+]H[−] = H[+]L[−] = H[+], and H[−]L[+] = L[+]. For condition 1, where the individual receives positive evaluations from both evaluators, we will assume that the simple additive process applies; and for condition 5, where both evaluators give negative evaluations, we will assume that both of them are ignored so that this condition is equivalent to the O-S condition. Table 11 presents expected and observed $P(s)$ values for this model.

TABLE 11

SELF-MAXIMIZING MODEL EXPECTED AND OBSERVED $P(s)$ VALUES

	Condition	Expected $P(s)$	Observed $P(s)$	Discrepancy (|Exp. − Obs.|)
1	H[+]L[+]	.81	80	.01
2	H[+]L[−]	79	75	04
3	H[+]H[−]	.79	67	.12
4	H[−]L[+]	64	57	.07
5	H[−]L[−]	.62	42	.20

As can be seen from the data in table 11, the self-maximizing model does a particularly poor job of predicting the observed data. Only for condition 1, where we actually used the prediction of another model, is the predicted value acceptably close to the observed.

Finally, one may compare the relative strengths of effect of the high- and low-ability evaluators.[14] If we consider the estimates of .16 and .17 to be reasonably accurate for the high-ability evaluator, and the accurate estimate for the low-ability evaluator to be somewhere between .02 and

for suggesting this test, as well as for providing ideas incorporated in n. 9 above and elsewhere

[14] It should be noted before beginning this discussion that we are here going beyond what has been definitely demonstrated in the experiments. Our intent has been to construct models of more general applicability; that is, models which are useful in understanding processes of information utilization in natural settings which are more complex than our experimental setting. Just because natural settings are complex, factors other than those we treat as independent variables (ability of the evaluators and positive or negative evaluations) will operate, and some of these factors could be important enough to obscure or to alter the findings established experimentally.

.06, then it is evident that the high-ability evaluator is somewhere between three and nine times as effective as the low-ability evaluator in this situation. One simple interpretation of this finding is that experts are considerably more influential than nonexperts in determining an individual's estimate of his ability.

A more interesting interpretation of this finding relates to practices common in fields where evaluations of an individual's work must be made by others who may not be competent to evaluate it. Often the response to ambiguous or unreliable evaluative information is to increase the number of others who are asked to become evaluators. For example, when articles are submitted to professional journals for publication, it is common to refer them to one or more readers who have worked in the same areas as the author. Especially when it is difficult to find a single referee who is considered highly competent to evaluate, multiple opinions are sought. Or, in the case of school children, when the homeroom teacher has difficulty in making a clear assessment of a child's ability, opinions frequently are sought from a number of other teachers who may not have had nearly so much opportunity to observe the child or nearly so great a competence to evaluate his performance.

The large difference in our estimates of effect of the high- and the low-ability evaluators makes the routine practice of soliciting multiple evaluations from others of doubtful competence appear unsound. In these experiments, for example, if one adopts the model which did the best job of predicting data from the two-evaluator experiments, the simple additive model, then the effect of a single high-ability evaluator is equal to the effect of from three to nine low-ability evaluators. Especially in the case of the schoolchild, where the desire may well be to raise his self-evaluation, our estimates indicate that it could require uniformly positive evaluations from nine separate low-ability evaluators to equal the effects of a single high-ability evaluator. Of course, the empirical probability that nine individuals will give uniformly positive evaluations of any individual's performance is likely to be rather low.

In summary, we have proposed and tested six alternative models of ways in which individuals combine information from potential sources. The model which did the best job of predicting results of independent experiments was the simple additive model, and this model has the additional advantage of being conceptually the simplest. Two of our models are elaborations of ideas presented in Berger and Fisek (1970), and our results may be considered an independent test and partial confirmation of their conclusions. Some consequences of the model and of the parameter values determined in this research were considered; among them, that the practice of substituting multiple sources of ambiguous evaluation for a single source of reliable evaluation appears to be unsound, and that there is no evidence

in our data of the frequently postulated tendency to maximize the evaluation of one's own abilities.

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Extensions of the Mover-Stayer Model[1]

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A major drawback to the traditional Markov formulation of mobility processes is that it assumes population homogeneity with respect to transition behavior. This assumption is clearly violated in most instances of social mobility. In an attempt to relax the homogeneity requirement and still retain the essential character of a Markov process, Blumen, Kogan, and McCarthy (1955) developed the "mover-stayer" model in which heterogeneity is attributed to the presence of two types of persons who differ in their rates of movement. In the present paper, the mover-stayer model is generalized to permit a continuous distribution of persons by rate of mobility. The model is illustrated with simulated data and then applied to an analysis of interregional migration.

INTRODUCTION

Applications of Markov processes to the study of social mobility have commonly concluded with the observation that individuals differ in their transition behavior. Although the Markov model requires population homogeneity, transitions from an origin state rarely conform to this assumption. Some persons simply move more often than, or differently from, others This has been found with industrial mobility data (Blumen, Kogan, and McCarthy 1955), with intergenerational and intragenerational occupational mobility (Hodge 1966; Lieberson and Fuguitt 1967), and with geographic migration (Rogers 1966; Tarver and Gurley 1965).

The main difficulty derives from the Markov model having been constructed with repeated state changes by a single object in mind. In the analysis of social mobility, however, the movements of an entire population are at issue. If this population is heterogeneous in its transition behavior, then even if each individual were to satisfy the central assumption of a first-order Markov process—namely, that his probabilities of making particular transitions are determined solely by his present state and are independent of past history—the population-level process would not be Markovian.

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Attempts to relax the homogeneity requirement while retaining the essential Markov framework have led to research in two directions. In one approach, interest has focused on the construction of subpopulation matrices and on ways to categorize individuals that would permit the "within-category" variation in transition behavior to be reduced. Operationally, this has usually meant disaggregating the population on attributes which are expected, either from theoretical considerations or empirical investigation, to relate to mobility and to construct a separate transition matrix for each subpopulation. For example, Rogers (1966) and Tarver and Gurley (1965), analyzing geographic migration, disaggregate the population to produce transition arrays by age categories and race. In this spirit, McFarland (1970) has reported an analytic method for combining subpopulation or individual-level transition matrices and projecting from these to the k-step population matrix, and I have (Spilerman 1972) presented a regression procedure for disaggregating the population matrix in order to obtain the individual-level transition arrays.

The above strategy casts the problem of heterogeneity into a framework in which each person is viewed as making a single transition during a unit time interval, but following a matrix relevant to the subpopulation that shares his particular attributes. A conceptually different approach to heterogeneity is embodied in an alternative assumption, namely, that all individuals move according to an identical transition matrix when they move but differ in their *rates* of mobility (see Spilerman [1972] for a discussion on the convergence of these two perspectives). Work in this direction has resulted in the development of the "mover-stayer" model (Blumen, Kogan, and McCarthy 1955). Under the specifications of this process, heterogeneity is handled by postulating two types of individuals—stayers, who remain permanently in their states of origin, and movers, who are homogeneous in their transition behavior and therefore follow a Markov process with a common transition matrix. Several estimation methods for the parameters of the mover-stayer model have been developed by Goodman (1961).

Aside from the novel conceptual perspective provided by this model, which seems appropriate to the analysis of geographic migration or intragenerational occupational mobility where repeated moves can be made by a person, it has the advantage of not requiring individual-level attribute data (although parameter estimation can be improved if such information on the waiting time to transition is available [Goodman 1961]). Since much of our mobility data lacks significant detail at the individual level, the mover-stayer model can be applied where the construction of subpopulation transition matrices is not possible.

Although the mover-stayer model postulates two types of persons, this is done out of necessity for keeping the process mathematically tractable,

not because the authors genuinely believed that instances of population heterogeneity can generally be attributed to two types of persons. In fact, in their concluding chapter, Blumen, Kogan, and McCarthy (BKM) (1955) discuss strategies for extending the mover-stayer model to incorporate a wider range of heterogeneity in the rate of transition, although they do not develop such a generalization. An extension of the mover-stayer model in which the rate of individual mobility is specified by a continuous distribution is constructed in this paper. Following the mathematical presentation, the extension is applied to regional migration data.

THE MOVER-STAYER MODEL AND BKM'S COMMENTS ON GENERALIZATION

The Mover-Stayer Model

In their study of industrial mobility, BKM (1955, p. 62) report that calculations of k-step transition matrices from a Markov chain consistently underpredict the main diagonal elements of the observed k-step matrix. That is, if

$$P(1) = \begin{bmatrix} p_{11} & \cdots & p_{1m} \\ \vdots & & \vdots \\ p_{m1} & \cdots & p_{mm} \end{bmatrix}$$

is the observed one-step transition matrix, and

$$P(k) = \begin{bmatrix} p_{11}^{(k)} & \cdots & p_{1m}^{(k)} \\ \vdots & & \vdots \\ p_{m1}^{(k)} & \cdots & p_{mm}^{(k)} \end{bmatrix}$$

is the observed k-step transition matrix, then the k-step matrix predicted from a stationary Markov process, $\hat{P}(k) = P(1)^k$, will have main diagonal elements $(\hat{p}_{11}^{(k)} \ldots \hat{p}_{mm}^{(k)})$, which commonly have the property that $\hat{p}_{ii}^{(k)} < p_{ii}^{(k)}$ for $i = 1, \ldots, m$.

Although one might suspect that overtime change in the p_{ij} elements of $P(1)$ is responsible for this result, this is generally not the case. For example, Hodge (1966) reports similar findings with occupational mobility data, even though the $P(1)$ matrices he uses are time dependent. More formally, Hodge's analysis shows that if $P_t(1)$ for $t = 1, \ldots, k$ are *observed* one-step transition matrices for successive time intervals, and if $\hat{P}(k)$ is the predicted k-step array,

$$\hat{P}(k) = \prod_{t=1}^{k} P_t(1),$$

then the relationship between the main diagonal elements of $\hat{P}(k)$ and $P(k)$ may have the same structure as that described between $\hat{P}(k)$ and $P(k)$. The problem is not one of the p_{ij} elements of $P_t(1)$ changing over time for the population but, rather, that some persons are less apt to move than others in each time interval.

To contend with this situation, BKM suggest decomposing the population into movers and stayers,

$$P(1) = S + (I - S)M, \tag{1}$$

where S is a diagonal matrix containing as entries the proportion of persons in each origin state who remain there permanently, $I - S$ is a diagonal matrix with entries which indicate the proportion in a state who are potentially mobile, and M is the transition matrix for mobile individuals The assumptions of the mover-stayer model, then, are (*a*) a proportion of the population in each state that never moves, (*b*) the population which is mobile is homogeneous in its pattern of movement and follows a Markov process, and (*c*) the process is stationary. We therefore have for the predicted k-step matrix,

$$\hat{P}(k) = S + (I - S)M^k.$$

Follow-up work on this model (Goodman 1961) has been concerned primarily with deriving consistent estimators for the matrices S and M and testing hypotheses relating to the mover-stayer process. Conceptually, however, there is a need to develop models which incorporate a greater range of heterogeneity. Instead of postulating two types of persons, we should like a process which handles several types and, ideally, a continuous range of individual differences in the rate of movement. Blumen, Kogan, and McCarthy address this problem in their concluding chapter (1955, pp. 138–46). Since the extension developed here proceeds from their suggestion, I first present their remarks.

BKM's Comments on Extending the Mover-Stayer Model

Instead of requiring every person to make a fixed number of transitions in each time interval, we assume that transitions are random occurrences[2]

[2] A transition from state i to state i would be considered movement in this terminology For example, if the states of the system were geographic regions, an $i \to i$ transition would represent *intra*regional movement. Alternatively, we might speak of the expected number of *exposures* to movement as do BKM (1955, p. 139) but allow an individual to not move (an $i \to i$ transition) at an exposure

and that the rate of movement by an individual refers to his *expected* number of transitions, not to the actual number. This is conceptually reasonable, since an individual with rate equal to, say, three moves per unit time interval will not necessarily make this exact number of transitions in every time unit. He may make zero or one moves in some intervals, four or five in others. Over a long time period he will nevertheless average three moves per time unit. A formal way of stating this is to assume that individuals move in accordance with a Poisson process[3] with parameter value (expected number of moves per unit of time) $\lambda = 3$.

Consider for the moment only individuals with a common rate of movement equal to λ. The $P(1)$ matrix would then be given by

$$P(1) = \sum_{v=0}^{\infty} r_v(1) M^v, \tag{2}$$

where $r_v(t)$ is a Poisson probability,

$$r_v(t) = \frac{(\lambda t)^v e^{-\lambda t}}{v!}$$

(with $t = 1$ in equation 2), and indicates the proportion of individuals who are expected to make v transitions during the time interval $(0,t)$ from among those having a rate equal to λ, and M is the transition matrix followed at each move. We also assume that the $r_v(t)$ values are the same for all states of the process.

With the above specification, it is easy to show[4] that $P(1) = e^{-\lambda[I-M]}$ for any matrix M. This result is important because the k-step transition matrix $P(k)$ is now given by

$$P(k) = e^{-\lambda k(I-M)} = [e^{-\lambda(I-M)}]^k = [P(1)]^k.$$

[3] A precise specification of the (stationary) Poisson process is given by the following four assumptions (1) λ is constant over time (2) In an infinitesimal time interval Δt, at most one event can occur (3) The probability of an event in Δt equals $\lambda \Delta t$; the probability of no event in Δt equals $1 - \lambda \Delta t$. (4) The occurrence of an event during $(t, t + \Delta t)$ is independent of the past behavior of the process The derivation of the Poisson distribution from these assumptions is a straightforward procedure (see Feller 1957, p 400).

[4]

$$P(t) = \sum_{v=0}^{\infty} r_v(t) M^v = \sum_{0}^{\infty} \frac{(\lambda t)^v e^{-\lambda t}}{v!} M^v$$

$$= e^{-\lambda t} \sum_{0}^{\infty} \frac{(\lambda t M)^v}{v!} = e^{-\lambda t} e^{\lambda t M} = e^{-\lambda t[I-M]}.$$

The convergence of the infinite sum to $e^{\lambda t M}$ will hold for an arbitrary matrix M (Gantmacher 1959, p. 113).

Thus, relaxation of the fixed-number-of-moves assumption does not necessarily prevent the population-level process from being Markovian. In the particular case where transitions are Poisson events[5] and the population is homogeneous in its transition rate, the Markov requirement will, in fact, be satisfied.

Now assume that we have g types of persons who differ in their rates of mobility. Each individual, however, follows the same M matrix when making a transition. If a proportion q_1 of the population moves with rate λ_1, a proportion q_2 moves with rate λ_2, etc., we could write separate equations identical to equation (2) for each subpopulation. Alternatively, let $r_v(t)$ equal the expected proportion of the *total* population which makes v transitions during the interval $(0,t)$, irrespective of the individual mobility rates. Then, combining the coefficients of M^v from the separate processes, we have

$$r_v(t) = \sum_{i=1}^{g} q_i \frac{(\lambda_i t)^v e^{-\lambda_i t}}{v!}. \quad (3)$$

If we generalize this result from g types of persons to sampling from a continuous distribution $f(\lambda)$, we obtain

$$r_v(t) = \int_0^\infty \frac{(\lambda t)^v e^{-\lambda t}}{v!} f(\lambda)\, d\lambda. \quad (4)$$

Equation (4) says that the expected proportion of individuals who make v transitions in the time interval $(0,t)$ equals the sum of the products of two quantities: (*a*) the proportion of individuals with rate equal to λ, and (*b*) the probability that an individual with rate λ will make v transitions. The summation is taken over all possible values of λ, which is assumed to have a continuous density function.

Blumen, Kogan, and McCarthy develop the generalization of the mover-stayer model to this point (as does Bartholomew [1967, pp. 27–37] in a recent review of mobility models). In the next section, we present a solution to the proposed extension, provide an estimation procedure for the parameters of the model, and discuss strategies for testing whether the assumptions of the model are met by data.

AN EXTENSION OF THE BASIC MODEL

Assumptions and the Derivation

In order to extend the mover-stayer model, it is necessary to specify a form for $f(\lambda)$ in equation (4). Since we have little a priori knowledge

5 In fact, the assumption that the occurrence of transitions follows a Poisson process leads to a *continuous-time* Markov formulation.

about this distribution, we assume a very general family of curves and use the observed data to estimate parameters for the specific distribution. We do, however, restrict $f(\lambda)$ to be unimodal or decline exponentially. This seems reasonable, since several studies of mobility (Goldstein 1964; Lipset and Bendix 1959, p. 158; Palmer 1954, p. 50; and Taeuber, Chiazze, and Haenszel 1968, p. 46) report distributions of persons by number of moves which have these forms.

Specifically, we assume that $f(\lambda)$ can be approximated by a gamma density,

$$f(\lambda) = \frac{\beta^{\alpha}}{\Gamma(\alpha)} \lambda^{\alpha-1} e^{-\beta\lambda} \qquad \lambda > 0,\ \alpha > 0,\ \beta > 0, \tag{5}$$

where

$$\Gamma(\alpha) = \int_0^{\infty} y^{\alpha-1} e^{-y} dy.$$

The gamma distribution is a very general family of unimodal functions and is often assumed where the shape of the actual curve is unknown.

With this assumption regarding $f(\lambda)$, we obtain from equations (4) and (5) (see Chiang 1968, p. 49, for details on the integration)

$$r_v(t) = \int_0^{\infty} \frac{(\lambda t)^v e^{-\lambda t}}{v!} \frac{\beta^{\alpha}}{\Gamma(\alpha)} \lambda^{\alpha-1} e^{-\beta\lambda} d\lambda$$

$$= \frac{\Gamma(v+\alpha)}{v!\,\Gamma(\alpha)} \beta^{\alpha} t^{v} (\beta + t)^{-(v+\alpha)}. \tag{6}$$

Using the relation $\Gamma(\alpha) = (\alpha - 1)\Gamma(\alpha - 1)$, this result may be written as

$$r_v(t) = \binom{\alpha + v - 1}{v} \left(\frac{t}{\beta + t}\right)^{v} \left(\frac{\beta}{\beta + t}\right)^{\alpha}, \tag{7}$$

which is a negative binomial distribution. Thus, under the assumption that each individual's transitions follow a Poisson process, with the individual rates of mobility specified by a gamma density, the proportion of the population making v moves in $(0,t)$ will satisfy a negative binomial distribution.

Substituting this result into equation (2) yields for the one-step transition matrix,

$$P(1) = \sum_{v=0}^{\infty} r_v(1) M^v$$

$$= [p(t)]^{\alpha} \sum_{v=0}^{\infty} \binom{v+\alpha-1}{v} [q(t)M]^{v}, \tag{8}$$

where $p(t) = \beta/(\beta + t)$, $q(t) = t/(\beta + t)$, and $t = 1$.

Conditions for a Closed Form Solution to $P(1)$

Recall that for X, a scalar, and any real number α, $(1 - X)^{-\alpha}$ has the binomial expansion

$$(1 - X)^{-\alpha} = 1 + \alpha X + \frac{\alpha(\alpha+1)}{2!} X^2 + \frac{\alpha(\alpha+1)(\alpha+2)}{3!} X^3 + \dots$$

$$= \sum_{0}^{\infty} \binom{v+\alpha-1}{v} X^{v}, \tag{9}$$

where the condition for convergence of the infinite sum is $|X| < 1$. By extension, we write for the infinite sum of matrices in equation (8),

$$\sum_{v=0}^{\infty} \binom{v+\alpha-1}{v} [q(t)M]^{v} = [I - q(t)M]^{-\alpha}, \tag{10}$$

where I is the identity matrix. We now discuss the condition for convergence of equation (10) and the computation of the right side when α is an arbitrary real number, not necessarily integer valued.

Analogous to the condition on X in the scalar case (eq. 9), the requirement for convergence of the infinite matrix sum is that all eigenvalues of $q(t)M$ are less than one in absolute value (Gantmacher 1959, p. 113). Since M is a transition matrix, it is stochastic and its largest eigenvalue equals one. However, $q(t) = t/(\beta + t) < 1$ for finite t (since $\beta > 0$), and this ensures that the eigenvalues of $q(1)M$, in particular, will be strictly less than one.

Substituting the result of equation (10) into equation (8), we obtain for $P(1)$,

$$P(1) = [p(1)]^{\alpha} [I - q(1)M]^{-\alpha}. \tag{11}$$

A second consideration concerns the computation of $[I - q(t)M]^{-\alpha}$ when α is an arbitrary real number.[6] For an integer k and a nonsingular

[6] Since $[I - q(t)M]^{-\alpha} = ([I - q(t)M]^{-1})^{\alpha}$ and an inverse is defined only for a nonsingular array, this matrix power is defined only if the term in brackets is nonsingular. This condition, however, will be satisfied for $q(t) < 1$, i.e., for finite t.

matrix A, the matrix power A^k always exists. For an arbitrary real number α, we define A^α to be

$$A^\alpha = e^{\alpha \log A},$$

which will hold for nonsingular A (Gantmacher 1959, p. 240).

If the eigenvectors of A are linearly independent (which will be the case if A has distinct eigenvalues), then we can diagonalize A,

$$A = HDH^{-1}, \tag{12}$$

where D is a diagonal matrix with the eigenvalues of A as entries, and H is a matrix containing the eigenvectors of A as columns.[7] In this circumstance, $A^{-\alpha}$ can be written[8] as

$$A^{-\alpha} = e^{-\alpha \log A} = He^{-\alpha \log D}H^{-1} = HD^{-\alpha}H^{-1}, \tag{13}$$

where $D^{-\alpha}$ is a diagonal matrix with elements of the form $\mu^{-\alpha}$. Consequently, letting $[I - q(1)M] = A$ in equation (11), $P(1)$ can be computed from M by the diagonalization procedure of equation (13).

Obtaining the M Matrix from $P(1)$

Equation (11) shows how we can obtain the population transition matrix if we know M, the individual level transition matrix, and α and β, the parameters of $f(\lambda)$. Unfortunately, our problem is generally the reverse of this situation. Given an observed population transition matrix $P(1)$, and estimates of α and β from the empirical distribution of the number of moves by an individual, we wish to obtain M so that equation (11), rewritten in general form, may be used to project to $P(t)$ for some $t > 1$.

No difficulty is encountered in solving equation (11) for M as long as the main diagonal elements of $P(1)$ are greater than one-half. We obtain

$$M = \frac{1}{q(1)} \{I - p(1)[P(1)]^{-1/\alpha}\}. \tag{14}$$

The requirement on the p_{ii} entries constitutes a sufficient (though not necessary) condition for ensuring the uniqueness of M.[9] When this re-

[7] If A does not have linearly independent eigenvectors equal in number to its order (which may be the case if the eigenvalues are not distinct), then A cannot be diagonalized It can, however, be put in Jordon form (Bellman 1960, p. 191) which creates computational difficulties but, frequently, not theoretical ones. With real data, it is rare that the eigenvalues are not distinct, so only the case where $[I - q(t)M]$ can be diagonalized is considered in this paper.

[8] The eigenvalues of D may be complex numbers, in which case $\log \mu = \log r + i\theta$, $-\pi < \theta \leqslant \pi$, where r and θ are the polar form components of the eigenvalue μ. To guarantee uniqueness the principal branch of the logarithm is used.

[9] An analogous restriction is necessary to obtain the matrix of transition intensities,

quirement is not satisfied multiple solutions may exist to equation (14). In this situation, if individual histories are available, the researcher has an option of either reducing the real-time duration of the unit time interval so that the condition on $P(1)$ will be met, or calculating M directly from the observed transitions.

Because most mobility processes operate "slowly" relative to frequency of sampling, the restriction on the p_{ii} entries is not a particularly severe one, even in the absence of individual-level data. Where it is satisfied, M can be obtained by diagonalizing $P(1)$ in the manner discussed previously

$$[P(1)]^{-1/\alpha} = Ke^{-(1/\alpha)\log Q}K^{-1} = KQ^{-1/\alpha}K^{-1},$$

where Q is the eigenvalue matrix of $P(1)$, and the columns of K are the corresponding eigenvectors Equation (14) therefore provides a method for estimating M from the population-transition matrix $P(1)$, under the assumption that population heterogeneity in the rate of movement can be specified by a gamma density.

The remaining parameters of the model, α and β of the gamma distribution, can be estimated directly from observed data on the number of moves by an individual. If $\bar{v}$ and S_v^2 are the sample mean and variance of this variable, then estimates of α and β can be obtained in terms of these values from the mean and variance formulas for a negative binomial variate (Chiang 1968, p. 50). This yields

$$\left.\begin{aligned} \hat{\beta} &= \frac{\bar{v}}{S_v^2 - v} \\ \hat{\alpha} &= \hat{\beta}\bar{v} \end{aligned}\right\} \qquad (15)$$

Projection

Having computed M, we can now project forward in time to find $P(t)$ the t-step transition matrix

$$P(t) = \left(\frac{\beta}{\beta + t}\right)^{\alpha}\left[I - \left(\frac{t}{\beta + t}\right)M\right]^{-\alpha}. \qquad (16)$$

This result will hold for finite t. For very large t, $q(t) \approx 1$ and $|I - q(t)M| \approx |I - M|$, which is singular since M is stochastic. Equation (16) is not defined in this circumstance, but other considerations (see Appendix A) suggest that $\lim P(t) = M(\infty)$, the equilibrium matrix for M, which may be found by the usual Markov methods.[10]

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corresponding to a continuous-time Markov process, from a probability transition matrix (see Coleman 1964, p. 181) I am indebted to Burton Singer for an enlightening conversation on this subject.

10 This assumes an absence of $\lambda = 0$ individuals. If stayers are present, then $\lim P(t)$ is given by the mover-stayer formulation (BKM 1955, pp. 111–14), $\lim P(t) = S + (I - S)M(\infty)$.

If we view the process as embedded in continuous time so that noninteger values of t are meaningful, these can also be used with equation (16). We are not restricted to multiples of the initial time period, since it is the distribution of the population with respect to the expected number of transitions that is changing and this change is continuous. By any time t, of course, each person will have made an integer number of transitions. Likewise, no mathematical difficulty is presented with projecting backward in time. Starting with $P(1)$, for example, we can find $P(1/2)$.[11] This flexibility is useful because we can often obtain better estimates for α and β after a sizable number of moves have been made. The projections of the model could then be compared with transition matrices for shorter time intervals.

Testing the Model

At the outset, we assumed that the matrix $P(t)$ could be written in the form presented in equation (2), and consequently that $r_v(t)$, the proportion of the population expected to make v transitions by time t, is the same for all system states. Blumen, Kogan, and McCarthy (1955, p. 139) also make this assumption in their comments on extending the basic mover-stayer model. A necessary condition for this requirement to be satisfied is that the individual rates of transition not be a function of the state a person is in Where data are available this assumption can be tested by computing the distribution of waiting times to a transition separately for individuals in each of the system states, and comparing these distributions.

There are two other assumptions of the model which can be tested—that individuals move in accordance with a Poisson process, and that the gamma density provides a reasonable approximation to the distribution of mobility rates in the population. To test one of these, we must, however, assume the validity of the other. If we assume that transitions are Poisson events, we can test the gamma specification by comparing the fit of the negative binomial estimates from equation (7) with the actual distribution of the number of moves. If the fit is poor, then the gamma assumption for $f(\lambda)$ in equation (5) should be altered and the Poisson compounded with a

[11] One referee suggested the following interpretation for this procedure. Consider an individual who is in state i at time 0, state j at time 1/2, and state k at time 1. In arriving at equation (14), the information that his intermediate state is j was discarded, only the information that he made two moves from time 0 to time 1, beginning in state i and terminating in state k, is used. How, then, can $P(1/2)$ be reconstructed? The argument is that because of the assumption of an identical transition matrix for every person and a continuous distribution of mobility rates, there are other individuals who move just like him, only more slowly, who will be in state i at time 0, state j at time 1, and the information they contribute to $P(1)$ serves as a proxy for the information he would have contributed to $P(1/2)$.

different curve.[12] Alternatively, repair work can be done on the gamma distribution (see discussion on the spiked gamma in the following section).

There are direct tests for whether the occurrence of events conforms to a Poisson process, such as on the interarrival times, but they assume an absence of heterogeneity. If a large number of interarrival times were available for each person, we could test the Poisson assumption separately for each individual without concern for the distribution of λ in the population. Most social data are not so rich in detail; consequently, it is necessary to assume the correctness of $f(\lambda)$ in order to test the Poisson assumption. Therefore, if we believe $f(\lambda)$ to be gamma, a comparison between the actual distribution of moves and the negative binomial estimates would provide a test of the Poisson specification. In practice, however, the form of $f(\lambda)$ would seem to be more problematic and the more interesting question.

AN EXAMPLE USING SIMULATED DATA

The advantage of illustrating the model with constructed data is that we have full knowledge of the actual mobility characteristics of the hypothetical population. We will assume an individual-level transition matrix and a population distribution by rate of movement as presented in table 1. In practice, this information usually would not be available.

TABLE 1

STRUCTURE OF THE SIMULATED DATA

A Individual-Level Transition Matrix

$$M = \begin{bmatrix} .600 & .200 & .100 & 100 \\ .150 & .700 & .100 & .050 \\ .100 & .100 & .750 & .050 \\ .050 & .050 & .100 & .800 \end{bmatrix}$$

B Distribution of the Population by Rate of Mobility

λ	Proportion of the Population with This λ Value
0.1	0.25
1.0	0.35
2.0	0.20
3.0	0.10
4.0	0.06
5.0	0.04
	1.00

We further assume that the six types of persons in the population (table 1, part B) move in accordance with a Poisson process which is specified by the indicated λ value for each subpopulation. Consequently, the Poisson

[12] If the fit is highly deficient, or if the assumption in the preceding paragraph is untenable, an individual-level transition matrix M which satisfies equation (8) may not exist. In this case, a nonstochastic M will be obtained from equation (14).

distribution was used to generate an expected proportion of each subpopulation who make $v = 0, 1, 2, \ldots$ moves during the time interval $(0,1)$. These values, multiplied by the respective subpopulation proportions in the total population, were aggregated to produce a distribution of the total population by number of moves. This distribution is presented in column 1 of table 2. Each of the $r_v(1)$ values has been multiplied by 1,000

TABLE 2

DISTRIBUTION OF NUMBER OF MOVES FROM OBSERVED (SIMULATED) DATA AND FROM NEGATIVE BINOMIAL ESTIMATES

v	$[1000\, r_v(1)]$	$[1000\, \hat{r}_v(1)]$
Number of Moves	Number of Persons with v Moves (Observed Data) (1)	Number of Persons with v Moves (Calculated from Negative Binomial $a = 1.371$, $\beta = 915$) (2)
0	388	363
1	226	260
2	153	161
3	97	94
4	59	54
5	34	30
6	19	17
7	11	9
8	6	5
9	2	3
10	1	1
$1{,}000 \sum r_v(1)$	996*	997*

NOTE $\bar{v} = 1.498$, $S_v^2 = 3.133$

* Value is less than 1,000 because of rounding error

so we can refer to the number of persons making a specified number of moves. These "observed" data were then used with the equation

$$P(1) = \sum_{v=0}^{10} r_v(1) M^v$$

to generate an "observed" transition matrix $P(1)$. This process was repeated[13] for the intervals $(0,3)$ and $(0,6)$, so we have three observed transition matrices: $P(1)$, $P(3)$, and $P(6)$. The matrices produced by this construction are presented in table 3. Normally, these transition arrays and the distribution of the population by number of moves (table 2, col. 1)

[13] The upper limit on the summation was set to 30 for $t = 3$ and to 60 for $t = 6$.

TABLE 3

OBSERVED TRANSITION MATRICES FOR THE TIME INTERVALS (0, 1), (0, 3), AND (0, 6)

$P(1) =$	.650	.156	.101	.093
	.118	.719	.101	.063
	.090	.102	.747	.061
	.056	.064	.101	.779
$P(3) =$	.448	218	.178	.155
	.165	529	.178	.128
	.143	.177	555	.125
	.110	.135	178	.577
$P(6) =$	.365	.235	.215	.184
	177	.441	215	166
	.164	.210	462	.164
	141	.181	215	463

TABLE 4

POPULATION TRANSITION MATRICES PREDICTED FROM A STATIONARY MARKOV PROCESS

$\hat{P}(1) = P(1) =$	.650	155	101	093
	.118	.719	.101	063
	.090	.102	747	.061
	056	064	.101	779
$\hat{P}(3) = [P(1)]^3 =$	346	261	209	183
	.199	.446	209	.147
	170	209	478	143
	.126	.154	209	.511
$\hat{P}(6) = [P(1)]^6 =$	.231	.280	.264	.224
	.211	.317	264	.207
	200	.260	337	.203
	.174	.224	264	.336

are the kinds of data which can be collected.[14] We now assume that only this information is available.

Projections from a stationary Markov chain require that the observed $P(1)$ matrix be raised to the requisite power. Markov estimates of $P(3)$, $\hat{P}(3) = [P(1)]^3$, and of $P(6)$, $\hat{P}(6) = [P(1)]^6$, are presented in table 4 As a result of the heterogeneity in rate of movement which was built into the data the main diagonal elements from this projection are, as expected,

14 We also have available the distribution of $r_v(t)$ for $t = 3$, 6, analogous to col 1 of table 2. This information will not be used in the illustration, but a test of the Poisson assumptions could also be based upon the change over time in the distribution.

consistently smaller than the corresponding observed values reported in table 3. Moreover, the discrepancy increases over time.

Turning to the model proposed in this paper, $\bar{v}$ and S_v^2 (from table 2, col. 1) were first used with equation (15) to estimate α and β: $\hat{\alpha} = 1.371$, $\hat{\beta} = 0.915$. Having estimated these parameters, the negative binomial formula (eq. 7) can be used to generate a predicted distribution of moves. This distribution is presented in column 2 of table 2, alongside the observed values. While there are some sizable deviations between expected and observed figures, this method will usually produce a superior fit than simply dichotomizing the population into stayers and movers,[15] especially when the heterogeneity is considerable.

The estimates of α and β, together with the observed $P(1)$ matrix from table 3, allow the M matrix to be derived using equation (14),

$$\hat{M} = \begin{bmatrix} .613 & .191 & .099 & .098 \\ .144 & .707 & .099 & .050 \\ .097 & .099 & .754 & .050 \\ .050 & .050 & .099 & .801 \end{bmatrix}.$$

This array is an estimate of the individual-level transition matrix M which, by the assumptions of the model, is the same for all persons. Equation (16) can now be used with $\hat{M}$, $\hat{\alpha}$, and $\hat{\beta}$ to project to $P(t)$ for any value of t. Estimates of $P(1)$, $P(3)$, and $P(6)$ are presented in table 5.

A comparison of these predicted arrays with the observed transition matrices (table 3) and with the Markov projections (table 4) reveals the superiority of the present model. The main diagonal entries, in particular, decline less rapidly than in the Markov projections.[16] The deviations from observed values, incidentally, cannot be attributed to any inadequacy with the Poisson assumptions, since these were used to generate the data. Rather, the deviations result from an inability of the gamma density to fit perfectly the constructed distribution of λ values in the population, although the discrepancy is not severe. (Compare the negative binomial estimates with the observed distribution of moves in table 2). In fact, using the computed values of α and β, the gamma density $f(\lambda)$ for this population can be drawn directly from equation (5). This graph is presented in figure 1. Superimposed on the curve are vertical lines which indicate the points of

[15] Nevertheless, the inadequacy of the mover-stayer dichotomization for data analysis can be less severe than would at first appear. As BKM (1955, p. 142) point out, it is not the case that all movers need make a single transition during a time unit, only that they follow a Poisson process with a common λ value A generalization of the mover-stayer model in which permanent stayers are permitted, as in the original formulation, is discussed in conjunction with the "spiked gamma" (with vodka, please).

[16] Estimates of the main diagonal entries from the present model converge to the equilibrium values predicted by the Markov model. At any finite t, however, estimates from the model herein are greater than the corresponding Markov values.

TABLE 5

POPULATION-TRANSITION MATRICES PREDICTED FROM THE GENERALIZED MOVER-STAYER MODEL*

$$\hat{P}(1) = \begin{bmatrix} .650 & .156 & .101 & .093 \\ .118 & .719 & .101 & .063 \\ .090 & .102 & .747 & .061 \\ .056 & .064 & .101 & .779 \end{bmatrix}$$

$$\hat{P}(3) = \begin{bmatrix} .423 & .232 & .184 & .162 \\ .175 & .510 & .184 & .131 \\ .150 & .184 & .539 & .127 \\ 113 & .138 & .184 & .565 \end{bmatrix}$$

$$\hat{P}(6) = \begin{bmatrix} .319 & .256 & .229 & .197 \\ .193 & .404 & .229 & .174 \\ 176 & .224 & 428 & .171 \\ .148 & .187 & 229 & 434 \end{bmatrix}$$

* Estimates are from equation (16).

concentration of the simulated data (from table 1, part B). It is apparent that the heterogeneity in the population is reasonably well represented by this gamma density, although many real social processes will actually permit a better fit than the arbitrary distribution constructed here (e.g. Spilerman 1970, table 4).

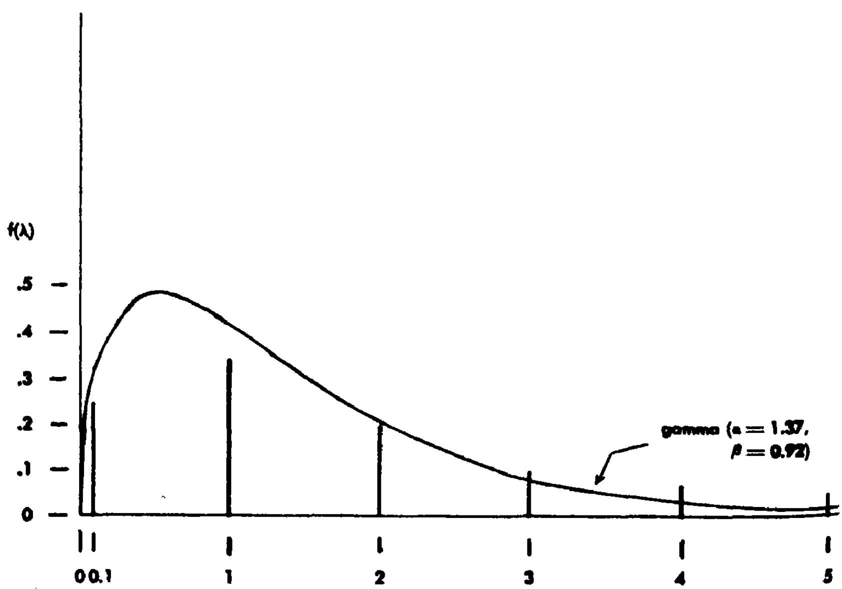


FIG. 1.—Distribution of the population by expected rate of movement, from simulated data.

Spiked Gamma

If the comparison between the negative binomial estimates and the actual distribution of the population by number of moves is less than satisfactory (as judged, for example, by a χ^2 test), the best recourse would be to compound the Poisson with a different family of curves which might allow a more adequate fit to $f(\lambda)$. However, if the gamma assumption fails in the way it is most likely to, by a very heavy concentration of nonmovers, this defect can be repaired by use of the "spiked gamma."

The spiked gamma is a direct generalization of the mover-stayer model, since, unlike the previous extension, the presence of permanent stayers ($\lambda = 0$ persons) is permitted. It is an extension of the mover-stayer model in that heterogeneity is allowed among the movers, who are assumed to be distributed by rate of movement according to a gamma density.[17] One procedure for estimating the parameters of this process would be to first apply a mover-stayer method to separate out stayers from the remainder of the population, then treat movers according to the present model. In fact, having removed stayers, a quick test of the need to even use the model of this paper, rather than the simpler Markov methods, can be obtained by comparing the mean and variance of the distribution of moves. For a Poisson distribution, var $(v) = E(v)$, while, if heterogeneity is present in the distribution of λ, var $(v) > E(v)$. Thus, by comparing $\bar{v}$ and S_v^2, the likely adequacy of a Poisson (mover-stayer) assumption for the movers can be ascertained. (See Spilerman [1970, p. 633] for a lengthier discussion and application of this point.)

An alternate procedure for estimating the size of the spike would be to assume that a gamma density provides the correct distribution of movers and choose that division of the population failing to move which minimizes the deviations of the observed values from the expected distribution of moves. Thus, we would use the gamma to estimate the number of *movers* who happen to make zero transitions during the interval (0,1). The advantage of this approach is that it will allow a "best fit" of the gamma to the distribution of movers who make $v \geqslant 1$ moves to be obtained, since the term for $v = 0$ does not influence the parameter estimates.

The procedure here is to fit a negative binomial to the observed distribution of moves (table 2, col. 1) except that information about the $v = 0$ term is not used. The "truncated" negative binomial[18] must be employed to

[17] In the BKM mover-stayer model, this gamma specification for movers is replaced by the more restrictive assumption that they are concentrated at a single λ point

[18] An analogous procedure, using the truncated Poisson distribution (Coleman 1964, p. 366), can be used to estimate the parameters of the BKM mover-stayer model.

estimate α and β when the zero term of the observed distribution is missing. This probability distribution is defined by the equation[19]

$$R_v = \frac{1}{1-p^a} \binom{\alpha + v - 1}{v} q^v p^a \qquad (17)$$

for $v \geqslant 1$, where the two independent parameters α and q are estimated by (see Appendix B)

$$\hat{q} = 1 - \frac{\bar{v}(1 - R_1)}{S_v^2},$$

$$\hat{\alpha} = \frac{1}{\hat{q}} [\bar{v}(1 - \hat{q}) - R_1].$$

In these formulas, $\bar{v}$ and S_v^2 are the sample mean and variance (with the $v = 0$ observation deleted from the computations), and R_1 is the proportion of the observed population with $v \geqslant 1$ moves who make a single transition. The remaining parameter of the gamma distribution, β, may be calculated from $\hat{q}$: $\hat{\beta} = (1/\hat{q}) - 1$.

These estimates of α and β are now used with the regular negative binomial formula (eq. 7) to estimate the number of *movers* who failed to move during the interval (0,1). The difference between these observed and calculated numbers, appropriately standardized,[20] provides an estimate for the size of the spike at $\lambda = 0$. Calculations using the alternate method were carried out with the data in column 1 of table 2. The results are presented in table 6, column 2, alongside the observed distribution. Because of the added degree of freedom in estimating the spiked distribution, the estimates are clearly superior to those obtained from the regular negative binomial.

With the size of the spike estimated by either method, we have a division of the population into stayers and movers and may project to $P(t)$:

$$P(t) = S + (I - S) \left(\frac{\beta}{\beta + t} \right)^a \left[I - \left(\frac{t}{\beta + t} \right) M \right]^{-a}. \qquad (18)$$

In this equation, S is a diagonal matrix containing as entries the proportion

[19] Equation (17) is not written as a function of time since only the values for $t = 1$ are considered here.

[20] The negative binomial estimates are standardized by forcing

$$\sum_1^\infty \hat{n}_v = \sum_1^\infty n_v,$$

where n_v is the observed number of persons making v transitions during the time interval (0, 1).

TABLE 6

DISTRIBUTION OF NUMBER OF MOVES FROM OBSERVED DATA AND FROM NEGATIVE BINOMIAL, WITH PARAMETERS ESTIMATED FROM TRUNCATED NEGATIVE BINOMIAL

v Number of Moves	$[1000\, r_v(1)]$ Number of Persons with v Moves (Observed Data) (1)	$[1000\, \hat{r}_v(1)]$ Number of Persons with v Moves (α=2.231, β=1.251 Calculated from Truncated Negative Binomial) (2)
0	388	388*
1	226	224
2	153	160
3	97	101
4	59	58
5	34	32
6	19	17
7	11	9
8	6	5
9	2	2
10	1	1
$1{,}000\, \Sigma\, r_v\, (1)$	996†	997†

NOTE.—$\bar{v}$ = 2 448, S_v^2 = 2 776, the v = 0 observation is excluded from these calculations.
* This value includes 162 persons in the spike
† Value is less than 1,000 because of rounding error.

of the initial population in a state who are stayers;[21] $I - S$ is a corresponding matrix for movers; and the remaining parameters are estimated as before, but now using only information on movers. The improved predictions for $\hat{P}(t)$, using equation (18), are presented in table 7.

GEOGRAPHIC MIGRATION

Using data made available by Karl Taeuber from his analysis of residential mobility in the United States (Taeuber, Chiazze, and Haenszel 1968), the model of this paper was applied to interregional transitions by males.[22] The Taeuber data were collected in 1958 from retrospective reports about prior residences and are described in detail elsewhere (Taeuber, Chiazze, and Haenszel 1968). For the purpose of this study, four geographic regions were defined as states of the process: (1) Northeast, (2) North Central,

21 Estimates of the proportion of stayers in each state are obtained directly when the mover-stayer estimation procedures are used. With the alternate method, one assumes that stayers constitute an identical proportion of the nonmovers in each state.

22 A computer program which performs the calculations associated with this model has been written by David Dickens. Copies of the program may be obtained from University Microfilms, Ann Arbor, Michigan (request item S-404).

TABLE 7

TRANSITION MATRICES PREDICTED FROM THE SPIKED GAMMA MODEL*

$$\hat{P}(1) = \begin{bmatrix} .650 & .156 & .102 & 093 \\ .118 & .718 & .102 & .063 \\ 090 & 103 & 746 & 061 \\ .056 & .064 & .102 & .778 \end{bmatrix}$$

$$\hat{P}(3) = \begin{bmatrix} .446 & .219 & .179 & .156 \\ .166 & 529 & .178 & .128 \\ 144 & .177 & .555 & 124 \\ 111 & .135 & .177 & .577 \end{bmatrix}$$

$$\hat{P}(6) = \begin{bmatrix} .371 & .232 & .215 & 183 \\ 175 & .448 & .213 & 165 \\ 163 & .207 & 468 & 162 \\ 142 & 180 & 211 & 468 \end{bmatrix}$$

* Estimates are from equation (18).

(3) South, and (4) West. The time points that were used are $t_0 = 1937$, $t_1 = 1944$, $t_2 = 1951$, and $t_3 = 1958$. These were selected to provide residence histories for the adult years of this cohort.

The data proved to be less than ideal for illustrating the versatility of this model to incorporate a wide range of heterogeneity. One difficulty is that the histories were collected only for the four most recent residences of an individual and for his residence at birth. Persons who have had more than five addresses, therefore, have gaps in their residence histories and had to be excluded from the analysis. Unfortunately, this meant that persons with high rates of mobility were deleted and consequently the heterogeneity in proneness to move was being artificially reduced.

A second difficulty with these data stems from the little interregional migration which appears to take place (see table 8, part B). In part, this derives from the truncation of the distribution at $v = 4$ moves, since individuals with many residence changes are most likely to have had some regional migration experience. However, it is also a consequence of the phenomenon we are examining. Apparently, persons do not change geographic region very frequently during a seven-year interval; indeed, only one out of five residence changes resulted in a move to a different region, using this four-category definition of region.

Table 8 presents the observed transition matrix for the population during 1937–44 (part A) and the observed and predicted distributions of the population by number of moves for this period (part B). In light of the above comments, these data pertain only to persons who made four or fewer residence changes during 1937–58. Comparing the observed distribution of moves (part B, col. 1) with the distribution predicted from the negative

TABLE 8

OBSERVED POPULATION TRANSITION MATRIX AND DISTRIBUTION OF MOVES FROM GEOGRAPHIC MIGRATION DATA, TOGETHER WITH DISTRIBUTION PREDICTED FROM NEGATIVE BINOMIAL

A. OBSERVED POPULATION TRANSITION MATRIX (1937–44)

$$P(1) = \begin{bmatrix} .970 & .010 & .012 & .008 \\ .007 & .947 & .015 & .030 \\ .011 & .028 & .938 & .023 \\ .003 & .015 & .017 & .966 \end{bmatrix} \qquad \begin{matrix} \underline{n} \\ 3437 \\ 4160 \\ 4110 \\ 1341 \end{matrix}$$

B. DISTRIBUTION OF THE POPULATION BY NUMBER OF MOVES DURING 1937–44

(v)	(n_v)	($\hat{n}_v$)
Number of Moves	Number of Persons with v Moves (1)	Number of Persons with v Moves (Calculated from Negative Binomial, $\alpha=1.771$, $\beta=6.382$) (2)
0	10,120	10,082
1	2,328	2,419
2	507	454
3	93	77
4	.	12
5	*	1
Σn_v	13,048	13,045

NOTE.—$\bar{v} = 0.278$, $S_v^2 = 0.321$

* Number not available

binomial (col. 2), it is evident that the fit is reasonably good, except at the tail end of the distribution. The difficulty at the tail probably results from a tendency to underreport moves when many were made. Remember that we are dealing here with recollections in 1958 of residences during 1937-44.

Using the $P(1)$ matrix from 1937–44 together with $\hat{\alpha}$ and $\hat{\beta}$ from column 2 of table 8, $\hat{M}$, the estimate of the individual-level transition matrix, was constructed using equation (14):

$$\hat{M} = \begin{bmatrix} .891 & .035 & .046 & .028 \\ .026 & .799 & .059 & .116 \\ .042 & .109 & .763 & .085 \\ .009 & .057 & .065 & .869 \end{bmatrix}.$$

Matrix $\hat{M}$, therefore, indicates how individuals transfer each time they move. It shows, in particular, that the Northeast is most successful in retaining its residents ***when they move***; the South, least successful. Using

the estimates of α, β, and M, we can now project to $P(t)$ for any time t. Ordinarily, equation (16) would be used for this purpose. However, to compensate for the effect of truncating the distribution, projection was accomplished using equation (8) with the upper limit of the sum set to four, the maximum number of moves by an individual retained for the analysis. As a result, negative binomial predictions of five or more moves have been deleted from the estimation of $P(t)$. The observed data, and projections obtained by using a Markov chain, as well as those by the present method, are presented in table 9 for the periods 1937–51 and 1937–58.

TABLE 9

OBSERVED AND PREDICTED TRANSITION MATRICES FOR 1937–51 AND 1937–58, FROM GEOGRAPHIC MIGRATION DATA

	$P(2)$—(1937–51)				$P(3)$—(1937–58)			
Observed matrices	.947	.015	.019	.019	934	017	.025	022
	008	923	.018	.050	009	.908	.020	063
	018	045	.907	030	020	052	890	038
	004	.023	019	.954	004	.024	.018	954
Projection from Markov process $[\hat{P}(t) = P(1)^t]$	942	.019	.023	016	915	.028	034	024
	014	.898	030	.059	.020	.853	043	085
	021	053	.881	.044	.031	.076	829	.064
	005	029	.032	.933	.008	043	047	904
Projection from present model* ($\alpha = 1.771$, $\beta = 6.382$)	946	.017	021	.016	.924	.024	.028	023
	.009	926	023	042	012	897	032	059
	018	033	912	037	.024	.046	878	052
	.008	.036	023	.932	001	.050	.032	.906

* $\hat{P}(t) = \sum_{v=0}^{4} \hat{f}(t) M^v$.

By comparing the main diagonal entries, especially for the 1937–58 matrices, it is evident that the model of this paper produces a superior fit to the data than is obtained from the Markov projection, although the latter estimates are themselves not poor. One reason, incidentally, why the predictions from both models are not better is because the stationarity requirement is violated. We are dealing here with a cohort through a 21-year period and extrapolating to a terminal year in which the population is 14 years older than at termination of the period used for parameter estimation. Thus, if age has an effect on migration behavior, as it surely does (e.g., Morrison 1967, pp. 558–59), we have a transition matrix which is changing over time. By comparison, BKM did not have this concern, since their data covered only a three-year period.

While the migration data preclude our demonstrating the suitability of

this model in situations of considerable population heterogeneity, we can investigate the reason why the Markov estimates are reasonably good. We are assuming here that transitions are Poisson events in which λ, the parameter of the distribution, varies over individuals. In this circumstance, an estimate of the variance of λ is given by $\hat{\sigma}_\lambda^2 = S_v^2 - \bar{v}$. Substituting the values of S_v^2 and $\bar{v}$ from table 8, we obtain $\hat{\sigma}_\lambda^2 = .043$, a value which is not very large. (By comparison, $\hat{\sigma}_\lambda^2 = 1.635$ for the simulated data in table 2.) Thus, primarily because of the deletion of individuals with more than four moves, little heterogeneity remains in the population, and the Markov chain model which formally requires all persons to have an identical parameter value now provides reasonably good projections. In fact, having estimated α and β, we can graph $f(\lambda)$ to ascertain the appearance of the heterogeneity in λ. This graph is presented as figure 2. Note that the scale

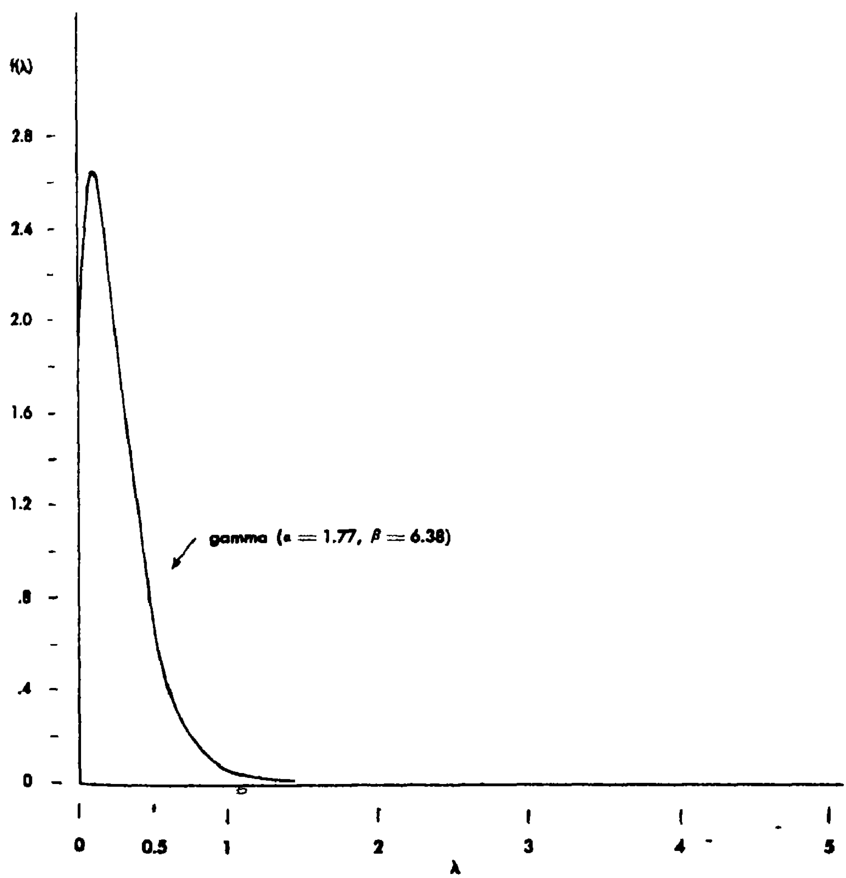


FIG. 2.—Distribution of the population by expected rate of movement, from geographic migration data.

unit on the Y-axis is one-half the size of the corresponding unit in figure 1. It is evident that the population is indeed highly concentrated over a narrow range of λ values.

CONCLUSIONS

The extensions developed in this paper, like the original mover-stayer model, cast the burden of explaining heterogeneity onto variations in the rate of mobility, since, by assumption, all persons follow an identical transition matrix at each move. If individual-level data on social characteristics are available, they can be used to determine the components of heterogeneity. Elsewhere (Spilerman 1970, pp. 646-48), I have argued that a regression methodology, in which the number of moves made by an individual is the dependent variable, is both consistent with this formulation and provides an approximation to analyzing the λ values themselves in terms of the independent variables.

While the emphasis here has been on the analysis of a stationary process, these methods will also shed light on the structure of time-varying processes. Data at two consecutive time points are required for parameter estimation in this model. Therefore, if the parameters are recalculated for adjacent time intervals of a time-varying process, we can ascertain whether the nonstationarity is primarily attributable to changes in the M matrix, which would suggest an alteration in the manner of selecting destination states at a transition, or to change in the gamma distribution, which would be indicative of a shift in the rate at which individuals are making transitions.

It is well known that the negative binomial distribution can be derived from an assumption of positive reinforcement (Coleman 1964, p. 300) as well as from this heterogeneity model. In the context of geographic migration, reinforcement would mean that, with each move, an individual's probability of making a subsequent transition is increased. Although this conceptualization seems forced, it becomes more plausible in an alternate formulation. Making a statement about the process by which moves occur is equivalent to making an assumption about the distribution of durations between the moves. Viewed from the latter perspective, reinforcement would suggest that the longer an individual resides at a particular location, the higher is his probability of remaining. Thus, the reinforcement hypothesis is recognizable as the "Axiom of Cumulative Inertia" in the Cornell Mobility Model (McGinnis 1968).

How do we distinguish between heterogeneity and reinforcement? Conceptually, one or the other is likely to be more appropriate to a particular phenomenon. For example, as McFarland (1970) has pointed out, the assumption that attachments grow over time seems more reasonable for geographic mobility than for occupational mobility. Analytically, it may

be possible to distinguish between these alternative processes by examining the change over time in the distribution of moves in successive time units. The reinforcement model suggests that the variance of this distribution should increase for a cohort as some individuals become increasingly prone to move. By contrast, the heterogeneity explanation suggests that the variance should remain constant. Nevertheless, in many social processes both phenomena probably occur, and individual-level attribute data would seem necessary in order to disentangle their separate effects.

APPENDIX A

$\lim_{t \to \infty} P(t) = M(\infty)$, The Equilibrium Matrix for M

PROOF

We assume M is ergodic and therefore

$$\lim_{v \to \infty} M^v = M(\infty)$$

exists (Feller 1957, p. 356). By definition,

$$P(t) = \sum_{v=0}^{\infty} r_v(t) M^v,$$

where $r_v(t)$ is specified by equation (7). For each v,

$$\lim_{t \to \infty} r_v(t) = \lim_{t \to \infty} \binom{\alpha + v - 1}{v} \left(\frac{t}{\beta + t}\right)^v \left(\frac{\beta}{\beta + t}\right)^\alpha = 0$$

since $\dfrac{\beta}{\beta + t} \to 0$. This implies that for $\epsilon > 0$ and L, an arbitrary integer, there exists a T such that for $t > T$,

$$\sum_{v=0}^{L-1} r_v(t) < \epsilon/2. \tag{A1}$$

Since $\lim M^v = M(\infty)$, we choose L such that for $v > L$

$$\max_{i,j} \left| m_{ij}^{(v)} - m_{ij}(\infty) \right| < \epsilon/2, \tag{A2}$$

where $m_{ij}^{(v)}$ is the (i,j) entry of M^v. Then, for $t > T$,

$$\max_{i,j} \left| p_{ij}(t) - m_{ij}(\infty) \right| = \max_{i,j} \left| \sum_{v=0}^{\infty} r_v(t) m_{ij}^{(v)} - \sum_{v=0}^{\infty} r_v(t) m_{ij}(\infty) \right|,$$

where $p_{ij}(t)$ is the (i,j) entry of $P(t)$, and the last sum follows from the relation,

$$\sum_{v=0}^{\infty} r_v(t) = 1.$$

Regrouping terms,

$$\max_{i,j} \left| \sum_{0}^{L-1} r_v(t)\ [m_{ij}^{(v)} - m_{ij}(\infty)] + \sum_{L}^{\infty} r_v(t)\ [m_{ij}^{(v)} - m_{ij}(\infty)] \right|$$

$$\leqslant \sum_{0}^{L-1} r_v(t) \max_{i,j} \left| m_{ij}^{(v)} - m_{ij}(\infty) \right| + \sum_{L}^{\infty} r_v(t) \max_{i,j} \left| m_{ij}^{(v)} - m_{ij}(\infty) \right|$$

$$\leqslant \sum_{0}^{L-1} r_v(t) \cdot 1 + \frac{\epsilon}{2} \sum_{L}^{\infty} r_v(t),$$

since, in the middle line, the maximum in the first summation is less than or equal to one because the matrices are stochastic, and the second maximum has been reduced using (A2). Using (A1) with the first sum in the final inequality and noting that

$$\sum_{L}^{\infty} r_v(t) \leqslant 1$$

in the second sum, we conclude that

$$\max_{i,j} \left| p_{ij}(t) - m_{ij}(\infty) \right| < \epsilon,$$

and therefore

$$\lim_{t \to \infty} P(t) = M(\infty).$$

APPENDIX B

The Truncated Negative Binomial Distribution

Let P_v be the negative binomial probability for v events:

$$P_v = \binom{\alpha + v - 1}{v} q^v p^\alpha, \qquad v = 0, 1, 2, \ldots . \tag{B1}$$

For any positive integer-valued distribution, we have

$$1 = \sum_{0}^{\infty} P_v = P_0 + \sum_{1}^{\infty} P_v,$$

and therefore,

$$\frac{1}{1-P_0}\sum_1^\infty P_v = 1.$$

This is a truncated distribution in that the P_0 term is lacking. For the negative binomial distribution, we obtain

$$R_v = \frac{1}{1-P_0} P_v = \frac{1}{1-p^\alpha}\binom{\alpha+v-1}{v} q^v p^\alpha, \qquad v \geqslant 1, \tag{B2}$$

which is the truncated negative binomial distribution. Note for reference that

$$R_v = \frac{\alpha+v-1}{v} qR_{v-1}. \tag{B3}$$

TO OBTAIN α

$$\begin{aligned}\mu &= \sum_1^\infty v\, R_v = R_1 + \sum_2^\infty v\, R_v \\ &= R_1 + \sum_2^\infty (\alpha + v - 1) q\, R_{v-1} \qquad \text{(by [B3])} \\ &= R_1 + q\sum_1^\infty (\alpha+v)R_v \\ &= R_1 + q\alpha + q\mu, \end{aligned} \tag{B4}$$

and $\alpha = (1/q)(\mu p - R_1)$.

TO OBTAIN q, β

$$\begin{aligned}\text{var}\,(v) &= \sum_1^\infty (v-\mu)^2 R_v \\ &= R_1 - \mu^2 + \sum_2^\infty v^2 R_v.\end{aligned}$$

Using (B3), we obtain for the last term,

$$\sum_2^\infty v^2 R_v = \frac{q}{1-q}[\alpha\mu + \alpha + \mu + R_1].$$

Substituting this value into the expression for var (v), and using (B4) to simplify, yields

$$(1-q)\,\text{var}\,(v) = \mu\,(1-R_1),$$

and

$$q = 1 - \frac{\mu(1-R_1)}{\text{var}\,(v)}.$$

Finally, since $q = 1/(\beta + 1)$ (from the definition of $q(1)$, eq. [8] in text), we obtain for β, $\beta = (1/q) - 1$.

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Some Problems in the Measurement of Class Voting[1]

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The relationship between class and voting has been described using concepts such as class voting, political distinctiveness of classes, class distinctiveness of parties, and class solidarity. Measures of these concepts are shown to rely on the same independent information. The Alford index of class voting can be interpreted as a regression coefficient. A model is developed to analyze under what conditions the index of class voting is affected by different ways of classifying occupational groups. The Gini index of segregation is considered as an alternative measure of the relationship between class and voting. Empirical examples are discussed.

The concept of class voting focuses on the relationship between class position and voting behavior, attempting to describe the nature of the relationship between the position of voters in the stratification system and their party preferences. This concept is thus of central importance to political sociology, where parties often are viewed as representing social classes, and elections sometimes are considered to be the democratic translation of the class struggle. In the literature of political sociology, we find other concepts besides class voting which attempt to capture and describe different aspects of the relationship between class and voting. These are concepts such as the political distinctiveness of classes, class distinctiveness of parties, class solidarity, and cross-class voting. This paper attempts to clarify the relationship between some of the concepts introduced in the literature and to analyze certain problems in the measurement of class voting.

CLASS VOTING AND RELATED CONCEPTS

Voting and class are often measured by indicators which approach the ordinal level of measurement. Political parties can be ordered on a Left-Right continuum. Sometimes this is difficult, for example, with parties based on religion, culture, or region. Occupations can be rank ordered, for example, in terms of prestige, although this also causes some difficulties.

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Both indexes of class voting which primarily have been used so far are based on dichotomies: the index of class voting suggested by Alford (1963, pp. 79–80) and the index of status polarization used by Campbell and his co-workers (Campbell et al. 1960, pp. 344–47). However, the index of status polarization is also applied when there are several categories within the independent variable.

Alford's index of class voting is computed as follows: "Subtract the percentage of persons in the non-manual occupations voting for Left parties from the percentage of persons in manual occupations voting for Left parties" (Alford 1963, pp. 79–80). This is thus the most common of measures: the percentage difference between the manual and nonmanual classes in votes for the left party.

The concept of status polarization is defined so that "variations in the status polarization of a society reflects variation in the intensity and the extent of class identification among its members. When polarization is high, most of the citizenry must have perceived a conflict of interests between strata and have taken on class identifications with fair intensity. When polarization is low, either few people are identifying, or extant identifications are weak, or both" (Campbell et al. 1960, p. 339). Although defined on the subjective level, Campbell et al. (1960, pp. 344–46) also use occupational categories as indicators of status polarization. As Alford (1963, p. 87) points out, this constitutes an ambiguity in their conceptualization. The difference in voting patterns between occupational groups—that is, class voting—should not be confounded with the relationship between subjective class identification and voting.

Campbell et al. (1960) give no single definition of the computation of their measure of status polarization. Instead, they illustrate the idea of status polarization with a figure showing three levels of polarization, from complete polarization to an intermediate stage to complete depolarization (Campbell et al. 1960, p. 345). Here, the measure of status polarization apparently is computed in the same way as Alford's index of class voting. In another context with more than two categories on the stratification variable, they use a coefficient of correlation (τ_b) as a measure of status polarization (Campbell et al. 1960, p. 347).

The concept of class voting refers to the difference between classes in their vote for the left party. Other related concepts are the political distinctiveness of a class—for example, the proportion of workers voting for the left party—and the class distinctiveness of a party—for example, the proportion of the left vote coming from workers (Alford 1963, p. 84). In countries such as Norway and Sweden, where the left party draws the votes of most workers as well as a considerable proportion of votes from higher strata, the political distinctiveness of workers is high but the class distinctiveness of the left party is relatively low. The British Labour

Party, on the other hand, receives votes from workers to a very large extent, but workers also vote for the Conservative Party in significant proportions. There, the class distinctiveness of the left party is high but the political distinctiveness of the working class is relatively low.

The concept of class solidarity is defined as "the degree to which lower or upper strata deviate from 50 percent in their two-party vote proportion" (Campbell et al. 1960, p. 348). Alford (1963, p. 83) defines a similar concept, relative class solidarity, as "the percentage point difference between the degree to which manual workers deviate from a 100 percent vote for the Left party and the degree to which non-manual persons deviate from a 100 percent vote for the Right party." This difference also gives the relative degree of "cross-class voting."

To avoid ambiguities, I will define the concepts referred to above in terms of the frequencies in table 1.

TABLE 1

ELEMENTS USED IN DEFINING RELATIONSHIP OF CLASS TO PARTY

	Party		
Class	Left	Right	Total
Working	a	b	$a+b$
Upper	c	d	$c+d$
Total	$a+c$	$b+d$	(N)

For simplicity, I will also use proportions rather than percentages in the following. The Alford index of class voting (CV) is then defined as $CV = a/(a+b) - c/(c+d)$. The political distinctiveness of the working class (PDW) is $PDW = a/(a+b)$, and that of the upper class (PDU) is $PDU = d/(c+d)$. The class distinctiveness of the left party (CDL) is defined as $CDL = a/(a+c)$. The class solidarity of the working class (CSW) as defined by Campbell is $CSW = a/(a+b) - 0.50$, while Alford's index of relative class solidarity (RCS) can be written as $RCS = b/(a+b) - c/(c+d) = d/(c+d) - a/(a+b)$. For the following discussion, it is helpful to define the political nondistinctiveness of a class—for example, the upper class ($PNDU$) as $PNDU = 1 - PDU = c/(c+d)$.

RELATIONSHIPS BETWEEN CONCEPTS

Measures of the concepts discussed above are related to each other since in table 1 we have only three pieces of independent information: the two marginal distributions and the frequency in one of the cells.

Before turning to the relationships between the concepts, let us first define two additional indices based on the marginal distributions in table 1 relating class to party. The proportion of left votes (PL) is $PL = (a + c)/N$ and the proportion of workers among the voters (PW) is $PW = (a + b)/N$. Together with PDW, which utilizes the frequency in one of the cells of table 1, these indices will be used to express the other concepts discussed above.

First, the political distinctiveness of the upper class is found to be

$$PDU = \frac{PW\,(PDW - 1) + 1 - PL}{1 - PW}. \qquad (1)$$

The level of class voting is equal to the difference between the political distinctiveness of the working class and the political nondistinctiveness of the upper class,

$$CV = PDW - (1 - PDU) = PDW - PNDU. \qquad (2)$$

The class distinctiveness of the left party depends on the political distinctiveness of the working class, the proportion of left votes, and the proportion of workers in the following way:

$$CDL = PDW\,\frac{PW}{PL}. \qquad (3)$$

The degree of class solidarity of the working class as defined by Campbell and his co-workers is

$$CSW = PDW - .50. \qquad (4)$$

Alford's index of relative class solidarity is found to be

$$RCS = PNDW - PNDU = PDU - PDW. \qquad (5)$$

Alford (1963, p. 83) states that the "level of relative class solidarity is determined by the absolute level of Left voting, not by the level of class voting." Equations (2) and (5) above indicate, however, that both relative class solidarity and class voting are dependent on the political distinctiveness of the classes and that relative class solidarity is related to class voting in the following way:

$$RCS = 1 + CV - 2\,PDW. \qquad (6)$$

Since we have only three pieces of independent information, the formulas above indicate some redundancy in the concepts. Once the proportion of left votes is given, relative class solidarity is dependent on the level of class voting except in the special case when the two classes are of equal size. Campbell's measure of class solidarity is neither informative nor useful since it only relates the political distinctiveness of a class to an

arbitrary baseline. On the other hand, the concept of class distinctiveness of parties carries independent information, since it is based not only on the political distinctiveness of a class but also on the two marginal distributions in table 1. This concept can therefore be used to compare parties.

FACTORS AFFECTING THE INDEX OF CLASS VOTING

The Alford index of class voting can be interpreted as a regression coefficient. If the distance between the two occupational categories is equal to unity, the difference between proportions of left votes from the manual and nonmanual classes gives the slope of the regression line of voting on class. The issue of class voting centers around the nature of the relationship between class and voting rather than on how much of the variation in voting can be explained by the stratification variable. A measure of regression is therefore more appropriate here than a measure of association (see Blalock 1964, pp. 50–52), such as the τ_b coefficient used by Campbell et al (1960, p. 347).

It has been noted that changes in the way of dichotomizing the population into classes may affect the value of the index of class voting (Kahan, Butler, and Stokes 1966). These effects may, however, sometimes be negligible (Alford 1967, p. 86). Let us examine under what circumstances the index of class voting is affected by splitting up the population into two classes.

For this purpose, a model with a continuous stratification hierarchy is assumed which is split at some point into a manual-nonmanual, or working class-upper class, dichotomy. Further, each political party is assumed to represent the interests of people located at a specific point in the hierarchy. Also, an individual's position in the stratification hierarchy is assumed to determine what Dahrendorf (1959, p. 178) has termed his latent interests. These latent interests are converted to manifest interests or political ideologies through processes that turn quasi groups into interest groups (see Dahrendorf 1959, chap. 5). There will usually be some (but not complete) overlap between latent and manifest interests, that is, between position in the stratification hierarchy and political ideology. The left parties represent the interests of persons lower in the stratification hierarchy more than do the right parties.[2] The location of the parties with respect to the stratification hierarchy may vary, however.

The situation above can be conceptualized in terms of the model underlying Coomb's unfolding technique (see, e.g., Torgerson 1958, chap. 14). This model is completely deterministic. The party choice of an individual is determined by his position on the stratification hierarchy and its loca-

[2] As far as the methodological argument in the following is concerned, this assumption can be reversed

tion with respect to the midpoint of the distance between the left and the right parties. Individuals located to the left of this midpoint vote for the Left, whereas those to the right vote for the Right. The trace line showing the probability for a left vote is a monotonic function of position on the stratification hierarchy. The probability for a left vote is unity up to the midpoint between the parties, where it falls to zero and remains so.

If one party wants to win a majority of the electors, it will try to move the crucial midpoint toward the other party. This is one way of arriving at the well-known prediction that the parties in a two-party system will adjust their policies toward each other (see, e.g., Downs 1957, chap. 8)

For simplicity, a rectangular distribution of the population along the stratification hierarchy is assumed. Let us also make the model probabilistic and assume trace lines of the type used in Lazarsfeld's well-known "latent distance" model (see, e.g., Torgerson 1958, pp. 374–85).

In the above model, the index of class voting will reach its *maximum value only when the midpoint between the parties coincides with the dividing line between the manual and the nonmanual class.* As soon as the population is dichotomized at some other point, the index will show a lower value. The amount of change in the value of the index will depend on the shape of the trace line and on the proportion of the population that falls between the point of the class dichotomy and the midpoint between the parties. Let us look at a few examples. Figure 1 and table 2 illustrate the effects on the index of class voting of different ways of dichotomizing the population when the shape of the trace line varies. The trace line depicted in Type A in figure 1 shows a very high degree of class voting with an index value of .80 when the manual-nonmanual dichotomy coincides with the midpoint between the strata (case 2). When increasing proportions of the population fall between the point of the manual-nonmanual dichotomy and the midpoint between the parties, and thus are misclassified in this sense, the value of the index decreases (cases 1, 3, and 4).

When the real level of class voting is lower, changes in the way of dichotomizing the population have similar but less pronounced effects. In Type B in figure 1, the maximum level of class voting is .40 (case 2) With the same changes in the way of dichotomizing the population as above, the value of the index decreases but does not drop off as rapidly as in the first situation (cases 1, 3, and 4).

If the midpoint between the parties divides the population in some other proportion than the 50-50 split assumed above, the effects of misclassification on the index of class voting will also depend on its direction With 30% of the population below the midpoint between parties and the maximum level of class voting at .80, 20% of the population classified into the working class gives an index of .70, while the same degree of error in classification in the opposite direction gives 40% of the population in the

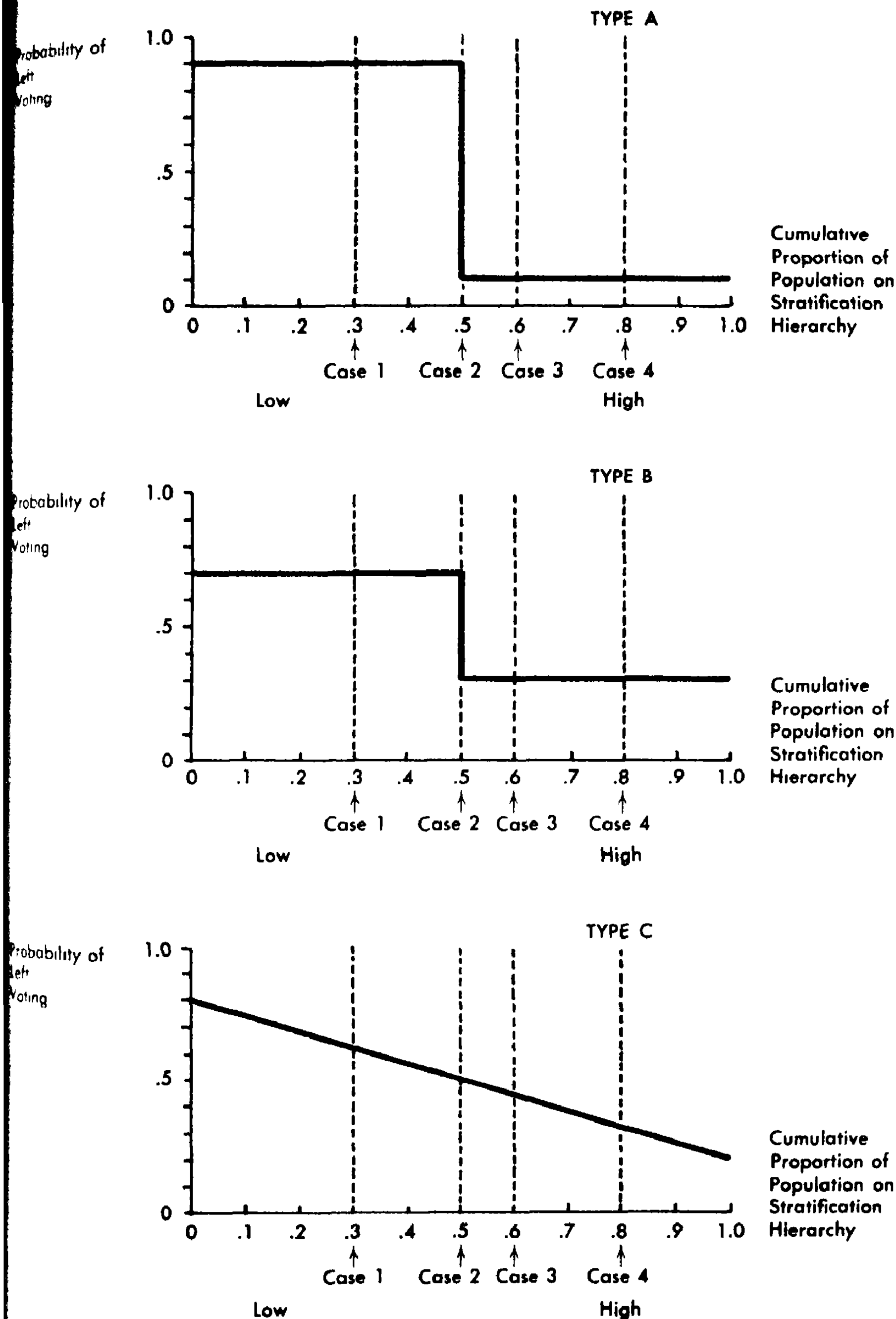


FIG 1—Three different types of trace lines, showing the relationship between probability for left voting and position on the stratification hierarchy with the rectangular population dichotomized at four different points (cases 1–4).

TABLE 2

INDEX OF CLASS VOTING AND CLASS DISTINCTIVENESS OF LEFT PARTY AS A FUNCTION OF THE FORM OF THE TRACE LINE AND THE PROPORTION OF THE POPULATION MISCLASSIFIED (CF. FIGURE 1)

	Proportion of Population in Working Class	Proportion of Population Misclassified	Proportion of Left Votes from Working Class (PDW)	Proportion of Left Votes from Upper Class (PNDU)	Index of Class Voting (CV)	Class Distinctiveness of Left Party (CDL)
Type A:						
Case 1	.30	.20	.90	.33	.54	54
Case 2	50	00	90	.10	.80	90
Case 3	60	.10	.77	.10	.67	92
Case 4	80	30	.60	10	.50	96
Type B.						
Case 1	.30	.20	70	41	.29	42
Case 2	.50	00	.70	30	40	70
Case 3	.60	.10	.63	.30	.33	.76
Case 4	.80	.30	.55	30	25	88
Type C:						
Case 1	.30	...	70	40	.30	.42
Case 2	.50	...	.65	.35	30	65
Case 3	.60	...	.61	.31	30	72
Case 4	.80	...	55	25	30	88

working class and an index of .60. With the maximum level of class voting at .40, the corresponding values of the index of class voting will be 35 and .30.

As indicated above, the effects on the index of class voting of changes in the way of dichotomizing the population depend on the shape of the trace line. The more abrupt changes in the slope of the trace line, the more pronounced effects we can expect as a result from changes in dichotomization. When the trace line is linear, these effects on the index of class voting will disappear if the population has the rectangular distribution assumed here. This is illustrated in Type C. Here, the index of class voting has the same value for the different ways of dichotomization.

In the above three types, the proportion of votes for the left party has been set at .50. The class distinctiveness of the left party is then dependent only on the political distinctiveness of the working class and on the proportion of workers in the population. One may note in table 1 that the class distinctiveness of the left party (as eq. [2] indicates) increases with increases in the proportion of the population classified into the working class; the class distinctiveness of the right party is affected in a parallel way. This index is affected by changes in the dichotomization of the population even when the class voting index remains unchanged (Type C).

The shape of the trace line thus determines what effects changes in the

way of dichotomizing the population will have on the index of class voting. The concept of class voting implies that occupational categories may form coalitions, or turn into interest groups. To the extent that this has occurred, we cannot expect linear trace lines but trace lines that have a steeper slope around the dividing line between the classes.

Also, the assumption of a population with a rectangular distribution is unrealistic. In most countries, population distributions are skewed toward the lower end of the stratification hierarchy and with different degrees of variation in different countries. Even if the regression of voting on class is linear, the slope of the regression line (therefore, also the index of class voting) will be affected by changes in the way of dichotomizing the population (see Blalock 1964, pp. 119–24, for a discussion of this problem).

In nations without a genuine two-party system, the problem arises how to dichotomize the parties into two categories: Left and Right. This problem is particularly difficult in countries with agrarian parties. The way in which this dichotomization is done may have effects on the index of class voting Let us look at a simple case with three parties—Left, Center, and Right—that can be ordered on a left-right dimension. The trace line giving the probability of voting for a party as a function of the stratification hierarchy is assumed to be monotonically decreasing for the left party, monotonically increasing for the right party, and nonmonotonic with a maximum around the middle of the stratification hierarchy for the center party

Dichotomization will have the effect of changing the slope of the trace line for the party with which the center party is combined. A combination of the center party with the left party will cause the trace line of the new left coalition to decrease more slowly both toward the bottom and toward the top of the stratification hierarchy. Also, the location on the stratification hierarchy of the point where the trace line has its steepest slope may be changed. Such changes might be important for the index of class voting.

ON THE SIGNIFICANCE OF CHANGES IN CLASS DICHOTOMIZATION

In dividing the population into a manual-nonmanual or working class-upper class dichotomy, difficulties arise with two types of occupational categories. One type of problem concerns occupations which can be ranked into a stratification hierarchy, but whether the group is placed into the higher or the lower class is largely arbitrary. The second type of difficulty has to do with occupational groups difficult to fit into a unidimensional rank order.

Among occupational groups that fit reasonably well into a stratification hierarchy but form a marginal sector between the higher and the lower strata, are sales workers, nonsupervisory clerks, policemen, firemen, foremen, railway conductors, noncommissioned officers, factory guards, and

the like. In many Western European countries, these groups constitute roughly 10%–25% of the economically active population. They form an intermediary sector between manual workers and the higher strata in terms of class identifications and voting patterns (see, e.g., Kahan et al. 1966, Dahlström 1954, chap. 2).

How is the index of class voting affected by the way in which we place reasonable proportions of the lower white-collar groups into the different classes? Let us look at a few examples, using data from Great Britain (Kahan et al. 1966), Western Germany (Linz 1967, p. 287), New Zealand (Robinson 1967, p. 97), and Sweden (Official Statistics of Sweden 1965, p. 95; *Indikator* 1969).[3] As shown in table 3 the largest increase in the index of class voting is .05 while the largest decrease is —.09.

TABLE 3

CHANGES IN THE INDEX OF CLASS VOTING WHEN PARTS OF THE LOWER NONMANUAL OCCUPATIONS ARE PLACED IN THE HIGHER CLASS AND IN THE LOWER CLASS, RESPECTIVELY

Country	Lower Nonmanuals in Upper Class	Lower Nonmanuals in Lower Class	Difference	Proportion Lower Nonmanuals in Sample
West Germany	28	23	05	02
Sweden (1964)	.38	.47	— 09	31
Sweden (1967–69, men only)	.46	48	— 02	19
New Zealand	.50	.46	.04	.18
Great Britain	.50	.48	.02	10

Among occupational categories that are difficult to rank into a unidimensional stratification hierarchy, independent farmers probably are the most important group (Carlsson 1958, pp. 145–46). The primary sector engages about 5%–40% of the economically active population in Western European countries. Alford (1963, p. 72) excludes farmers from his study of class voting in the Anglo-American democracies. In many countries, however,

[3] The reclassified occupational categories are classified as Grade IV in the British data (lower nonmanual, including shop salesmen and assistants, policemen, caretakers, lodginghouse keepers, waiters, etc. Kahan et al. 1966, p. 131). The reclassified groups are referred to as "artisans" and "lower white collar" in the data from West Germany (Linz 1967, p. 287) and as "white collar" and "uniform" workers in the data from New Zealand (Robinson 1967, p. 97). In the Swedish election statistics from 1964, this group consists of "lower salaried employees, sales workers, etc.," while in the data from 1967–69 those reclassified are "lower middle class occupations"—for example, policemen, waiters, second line foremen, sales and office workers, and telephone operators (*Indikator* 1969, pp. 3–4). Note that the data from New Zealand are a sample from only a part of the country (Robinson 1967, p. 96).

farmers are such a sizable and relatively homogenous interest group that they can hardly be omitted from a study of voting patterns in the society.

How is the index of class voting affected if farmers are included in the class dichotomies? Since farmers generally do not vote for parties on the left, they should be placed in the nonmanual category. Data from Western Germany (Linz 1967, p. 287), from the 1948 and the 1952 presidential elections in the United States (Campbell et al. 1954, p. 72) and from Sweden (Official Statistics of Sweden, p. 97; *Indikator* 1969, p. 3–4) are used in table 4 to compute indexes of class voting when farmers are excluded and when farmers are placed in the nonmanual category.

TABLE 4

CHANGES IN THE INDEX OF CLASS VOTING WHEN FARMERS ARE INCLUDED IN THE NONMANUAL CLASS AND WHEN FARMERS ARE EXCLUDED

Country	Farmers in Nonmanual Class	Farmers Excluded	Difference	Proportion Farmers in Sample
United States (1952)	.23	.22	.01	.13
United States (1948)	.40	.24	.16	.17
West Germany	.31	.28	.03	.12
Sweden (1964)	.45	.38	07	.11
Sweden (1967–69, men only) ...	49	46	.03	.08

The change in the index of class voting when farmers are included is largest for the 1948 elections in the United States, when the farmers to a large extent voted for the Democrats. In the other cases, the index of class voting changes from .01 to .07 units.

In order to evaluate the significance of the changes in the above examples, we can compare them with the variation in the index values reported by Alford (1963, p. 102; 1967, pp. 81–82) in his study of the Anglo-American democracies. Alford established the following rank order between the countries based on the mean values of the index of class voting from several different surveys within each country: Great Britain, .40; Australia, .33; United States, .16; and Canada, .08. The difference in the mean index value between Great Britain and Australia is thus .07; between United States and Canada it is .08. Therefore, it appears difficult to argue that changes in the index of class voting caused by different ways of handling farmers and the lower nonmanual occupations are negligible.

SEGREGATION INDEXES AS MEASURES OF CLASS VOTING

It has often been the case in the social sciences that attempts to solve methodological problems in one specific area have not taken advantage of

advances made in solving similar problems in other areas. The problem of measuring class voting is related to problems in measuring inequality or concentration of distribution that have been encountered in economics, geography (see, e.g., Duncan, Cuzzort, and Duncan 1961), and political science (see, e.g., Alker and Russet 1966). Within the field of sociology, similar problems have been attacked in the measurement of racial segregation.

If both class and voting are measured at the nominal level, the problem of indexes for class voting becomes identical to that of measuring racial segregation in ecological studies. Several measures of segregation have been used. Their interrelationships and methodological properties have been analyzed by Duncan and Duncan (1955). They find that the information contained in the various segregation indexes is based upon the proportion nonwhite and the segregation curve. The segregation curve is obtained by arranging the ecological units in the order of increasing proportions of nonwhite and then computing unit by unit the cumulative proportions of nonwhites and whites. Then the cumulative proportion of nonwhites is plotted on the x-axis and the cumulative proportion of whites on the y-axis The result is the segregation curve. The amount of segregation is depicted by the area between the diagonal, which shows the situation of equal proportions of whites and nonwhites in all units, and the segregation curve This area can be expressed as a proportion of the total area under the diagonal, giving the Gini index (Kendall and Stuart 1958, pp. 48 ff.).

The segregation curve and the Gini index of segregation can obviously also be used in the study of political cleavage on occupational, religious, regional, or other bases.[4] The spatial units of ecological studies are then supplanted by the occupational groups (or whatever groupings of voters we may choose), while the proportion left (or right) votes takes the place of the proportion nonwhite.[5]

The values of the Gini index and the index of class voting for eight

[4] A problem with the Gini index is that it defines the absence of segregation as the situation when all units have *equal* proportions nonwhite rather than the situation when nonwhites are *randomly* distributed between units.

An alternative measure can be developed by examining the properties of pairs of individuals. A measure of "cross-cutting" based on the proportion of all pairs of individuals whose two members are similar in one property but different on the other property has been proposed (Rae and Taylor 1970, chap. 4).

[5] To compute the Gini index, occupational groups are rank ordered according to the cumulative proportions of left votes (x_i) and right votes (y_i) as in the segregation curve. Let $x_0 = y_0 = 0$. With k groups, the Gini index may be calculated from the following formula:

$$\text{Gini index} = 1 - \sum_{i=1}^{k} (x_i - x_{i-1})(y_i - y_{i-1}) - 2 \sum_{i=2}^{k} (x_i - x_{i-1})\, y_{i-1}.$$

TABLE 5

A COMPARISON BETWEEN THE INDEX OF CLASS VOTING AND THE GINI INDEX WHEN FARMERS ARE EXCLUDED AND WHEN FARMERS ARE INCLUDED IN THE NONMANUAL CLASS

	Farmers Excluded		Farmers Included		
Country	Index of Class Voting	Gini Index	Index of Class Voting	Gini Index	Number of Occupational Categories
Jnited States (1952)	23	.27	.22	.28	5
West Germany	.28	.35	.31	.45	11
Sweden (1964)	.38	48	.45	58	6
Jnited States (1948)	.40	.46	24	.47	5
Sweden (1967–69, men only)	.46	.57	.49	.61	9
New Zealand	50	59	..	..	5
Sweden (1946)	..	...	.49	53	3
Great Britain	. .	..	50	.52	6

lifferent cases are compared in table 5 both when farmers are included and vhen they are excluded.[6] On the whole, the two indexes rank order and group the cases in the same way when farmers are excluded. If we include armers, the greatest discrepancy is again found for the data from the 1948 lections in the United States.[7] When farmers are included, the relative legree of class voting in Sweden in the 1964 elections and in West Germany lepends to a considerable degree on what index we use.

One advantage of the Gini index of segregation is that it can be based on several occupational categories. Thereby more of the available information is utilized than is the case with the index of class voting. Also, it is possible to include occupational groups such as farmers and the lower nonmanual sections that are difficult to place unambiguously into a class lichotomy. The Gini index is theoretically relevant since it measures inquality in the distribution of votes between occupational groups that form he basis of the stratification system. To the extent that there is a unidimensional rank order between occupational groups on the stratification ariable, information is lost, however. On the other hand, this information s not utilized very effectively in a dichotomy either.

Problems with the definition of occupational groups remain. The Gini ndex is sensitive to the number of occupational categories used. In general, he higher the number of occupational subdivisions used, the higher index alues we can expect to get. If new occupational groups are created through

The data are from the same sources as those in table 2 and 3.

This comparison is in a sense "unfair" to the index of class voting, since the Gini ndex does not reflect changes in the direction of the farm vote between 1948 and 952, while the index of class voting is affected since farmers are placed in the higher ratum in both elections.

subdivisions of larger occupational categories, the value of the index cannot decrease but will increase if the new subdivisions show voting patterns different from the larger category. The effects on the index depend on the relative size and homogeneity of the new groups created. This is illustrated in the following example.

In the Swedish data from 1967–69, there are nine occupational categories with the following proportions of left votes: unskilled workers (.79), semi-skilled workers (.78), skilled workers (.78), workers in agriculture and forestry (.63), lower white collar (.53), self-employed entrepreneurs (.30), middle white collar (.20), higher white collar (.14), and farmers (.06). If only five occupational categories (manual workers, lower white collar, other white collar, self-employed entrepreneurs and farmers) are used, the Gini index decreases from .61 to .60. If however, the sizable lower white-collar category is grouped together with the other white-collar occupations, the Gini index drops to .50.

SUMMARY AND DISCUSSION

The concepts proposed to cover different aspects of the relationship between class and voting have often been defined only at a verbal level and have not been tied clearly enough to the three pieces of independent information available in table 1: the two marginal distributions and the proportion in one of the cells. Some of the concepts proposed are therefore redundant. With the two marginal distributions given, the index of class voting taps the remaining information. Therefore, concepts like class solidarity or cross-class voting do not give much additional information. The class distinctiveness of a party, however, gives information not covered by the class voting measure.

The regression curve of left voting on position in the stratification hierarchy is the most exhaustive and relevant indicator of the relationship between class position and voting. Where the stratification variable can be measured on the cardinal level (e.g., income or years of schooling) the regression curve can be used as an index of class voting.

The Alford index of class voting can be interpreted as a regression coefficient. Its value is affected by the way in which the population is dichotomized into classes. The seriousness of these effects depends on the shape of the regression curve of voting on class and on the proportion of the population that is misclassified. In most cases such effects will occur even if the regression of voting on class is linear. It appears difficult to argue that these effects are negligible.

The Gini index of segregation is relevant for the theoretical content of the class voting concept and has some advantages when compared with the Alford index. Since the Gini index uses several categories on the occupa-

tional variable, we can in general expect to have more reliable measurements with the Gini index. Occupational categories that are difficult to place unambiguously into a class dichotomy can be handled with the Gini index. Furthermore, it makes better use of available data. If positively skewed population distributions are dichotomized close to the median, the lower class will be much more homogeneous than the upper class. The high political distinctiveness of some of the nonmanual groups will then pass unnoticed.[8] However, the Gini index is not independent from the particular occupational groupings used and requires that the same definitions of occupational categories be applied in the countries compared.

Simplicity is the appeal and advantage but also the weakness of the Alford index of class voting. If it is to be used for comparative purposes, we should ideally have countries with two-party systems and with unidimensional systems of stratification. The four Anglo-American democracies studied by Alford probably come closest to this ideal. In many other countries, however, the relationship between the system of stratification and the party system is so intricate that the model becomes an undue oversimplification. This is illustrated by the problems associated with describing the level of class voting in a country with a sizable proportion of independent farmers and an agrarian party.

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[8] An example of such groups are military officers which in a Swedish study were found to support the bourgeois block to 96% and the most conservative party to 85% (Abrahamsson 1968).

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Crime and the Division of Labor: Testing a Durkheimian Model[1]

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This study provided an empirical examination of an unsubstantiated but generally accepted proposition found in Durkheim's *The Division of Labor in Society*. The proposition that deviance or crime will increase concomitant with an increasing division of labor has found wide acceptance in both deviance and organizational studies without having been empirically verified. In order to test this proposition and four corollary suppositions, an examination was made of collective life utilizing data similar to those relied on by Durkheim in his examination of the etiology of suicide. In Durkheimian terms, then, this study involved an examination of degrees of collective order (operationalized as crime rates) as found within different collectivities (communities) possessing differential degrees of functional integration (division of labor). The primary hypothesis was largely refuted, but alternative interpretations of the findings suggest that the model cannot be discounted.

The consideration of types of societies and their underlying bases of social integration has generally led to a simple dichotomy of types. Variously named, but invariably the same, the dichotomy consists of the communal type based on consensus and the corporate type based on differentiation. Similarly, two fundamental types of social integration have received most of the consideration from social theorists. The first, normative integration, is thought to be the basis of social order in the communal type society wherein integration or solidarity is maintained by universal consensus of the populace on a relatively consistent set of norms and values. Functional integration, on the other hand, is generally thought to prevail in the more complex or corporate type societies. The functional integration of an organization is based on a division of labor among its members which makes them mutually interdependent. This interdependence promotes and secures the solidarity or integration of the organization.

Formulating what is essentially a theory of societal evolution, Durkheim derived a similar dichotomy with his mechanical and organic types of

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solidarity, generally thought to be identical with normative and functional integration, respectively. In *The Division of Labor*, Durkheim posited that as a community grows in complexity, size, and density, the dominant mode of integration evolves from the mechanical to the organic type. This theory of societal evolution was meant to explain the growth of social organization and its parallel growth processes, especially the division of labor. Among the processes expected to occur concomitant with these changes were the collective conscience becoming more abstract; law, morality, and civilization generally becoming more rational; a decrease in normative consensus; and an increase in deviant behavior.

The authority of the common conscience is based largely on the authority of tradition, and under the conditions of mechanical solidarity there is a high degree of control over the individual. But as the moral community disintegrates, the sanctions of public sentiment and law are thereby weakened and the basis of integration evolves toward what Durkheim termed organic solidarity. As he stated, "It is the division of labor which more and more fills the role of what was formerly filled by the common conscience. It is the principal bond of social aggregates of higher types" (1964, p. 175).

Durkheim noted that greater individual variability is a necessary correlate of an increase in the division of labor. He contended that "the division of labor can progress only if individual variability increases" (1964, p. xx) In essence, the authority of the collective conscience diminishes and individual variability increases as the community moves in the direction of increasing size, heterogeneity, complexity, and an order of integration or solidarity based on functional or organic interdependence. As these phenomena progress the individual is freed from traditional authority, and much greater variability in his behavior becomes acceptable. As Durkheim stated, "In accordance with the effacement of the segmental type, society, in losing hold of the individual can much less hold divergent tendencies together" (1964, p. xx). Simply put, as the strength of the collective conscience dissipates and the community moves toward an organic or functional basis of integration, it can expect an incipient increase in individual variation or deviance.[2]

[2] Nowhere in *The Division of Labor* does Durkheim explicitly indicate that an increased division of labor or differentiation leads to a rise in the rate of crime His definition of crime, of course, precluded such a conclusion, for he conceived of crime as actions which offend strongly defined states of the collective or common conscience as represented in repressive law, and the violation of which engenders punishment rather than restitution (1964, pp. 80–132). Organic solidarity, however, is characterized by restitutive law and "these rules are more or less outside the collective conscience" (1964, p. 113). Furthermore, the collective conscience loses its intensity and diminishes as the society becomes more functionally integrated and thus, by definition, there would be less crime with increased differentiation. From Durkheim, "The acts which custom alone must repress are not different in nature from those the law

This, then, constitutes Durkheim's model of the interplay between the division of labor, organic solidarity, and deviance. It needs to be said, however, that these phenomena require an increased population size and greater social and physical density. As Durkheim argues, "The division of labor varies in direct ratio with the volume and density of societies, and, if it progresses in a continuous manner in the course of social development, it is because societies become regularly denser and more voluminous" (1964, p. 262). The interaction of these factors provide a mutually reinforcing scheme in which greater deviance can be treated as an expected consequence. Accordingly, deviance was considered here as the dependent variable, population size and physical and social density were considered as independent or predictor variables, and the division of labor was treated as an intervening variable. This conceptual model is presented below:

Population Size (X_2) →
Population Density (X_3) → Division of Labor (X_1) → Deviance (Y_1, Y_2)
Social Density (X_4) →

The following hypotheses were derived from this theoretical framework.

I. There is a direct relation between the extent of the division of labor in communities and the extent of deviance within these communities.
II. There is a direct relation between the number of people residing in communities and the extent of the division of labor within these communities.
III. There is a direct relation between the social density of communities and the extent of the division of labor within these communities.
IV. There is a direct relation between the physical density of communities and the extent of the division of labor within these communities.

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punishes; they are only less serious. . . . When the weakest sentiments lose their energy, the strongest sentiments . . . cannot keep theirs intact" (1964, p. 301).

Thus, if we extend these arguments to their logical conclusion, in time nothing will offend the collective conscience or else it will cease to exist and there will be no crime. Knowing full well that reported "crimes" were increasing with societal evolution (1938, p. 66), Durkheim ignored these flaws in logic and instead focused on what has been termed deviance or "individual variations." This problem could have been avoided if Durkheim had defined crime as an act in violation of a law, for he clearly recognized that the number and variety of special interest laws were increasing (1964, pp. 302–3), and also the opportunity for violating these laws would naturally increase with societal evolution (1964, p. 153). Thus the conceptual leap in this research from Durkheim's "individual variation" and "divergent tendencies" to deviance, which becomes operationally defined as crime.

V. There is an indirect relation between the size and physical and social density of communities and the extent of deviance within these communities.

These hypotheses were tested using the following operationally defined variables as epistemic correlates of the theoretical variables.

Deviance.—Deviance was operationally defined using two separate indices—the property and the personal or violent crime rates of a community. The property crime rate is composed of the number of "serious" property crimes, that is, burglary, larceny ($50 and over), and auto theft Composing the personal crime rate are the offenses of criminal homicide, forcible rape, robbery, and aggrevated assault. In each case the rate is composed of the number of crimes known to the police in each community as reported in the 1960 *Uniform Crime Reports.*

It can be argued that property crimes are a more valid index of basic structural changes—and by extension, social integration—than personal or violent crimes. As Clinard has noted, "the influence of functional changes in a society is reflected far less in personal crimes than in property offenses which involve the acquisition of things and not necessarily any personal or fortuitous situation" (1942, p. 204). The violent or expressive crime is generally spontaneous or an act of passion, committed without reflection or premeditation. The property or instrumental offense, on the other hand, is generally the result of rational and motivated behavior, and as such seems a more valid index of the offender's normative orientations, which are in turn a reflection of the underlying social structure.

Yet Durkheim did not provide an explicit rationale for the examination of only property offenses; rather, he dealt with crime or deviance as a generic concept. For this reason it was thought that testing the hypotheses using different operational indicants of the criterion would provide a more valid test of the conceptual model.

Division of labor.—Following Gibbs and Martin (1962) and Labovitz and Gibbs (1964), a definition of the division of labor was used which differs somewhat from that suggested by Durkheim. In essence, the division of labor refers to the degree of functional interdependence among population elements in regard to their sustenance and economic activities (Clemente 1972). Hauser has argued that "one of the most fruitful ways of getting at the sustenance and functional economic relations within a community and between communities lies in the analysis of the labor force of the community classified by industrial and occupational composition" (1956, p. 486). There are two basic components of the division of labor measure. First is the number of different industries found in a population,[3]

[3] Although important, this characteristic is of little concern in this study since a constant number of categories is used.

and second is the differential distribution of individuals within these industrial categories (Labovitz and Gibbs 1964, p. 6). The more even the distribution of individuals among the industrial categories, the greater the division of labor. For example, given 10 industries with 90% of the population in one category or 10 industries with 10% in each, the division of labor or industrial diversification is much greater in the latter case. As Labovitz and Gibbs have demonstrated, given an equal number of industries, "the more even the distribution among them, the greater the division of labor" (1964, p. 6).

The formula for the division of labor is $D = 1 - [\Sigma X^2/(\Sigma X)^2]$, "where D is the degree of division of labor for the population, and X is the number of individuals" in each industrial category. It is further indicated that "the minimum value of D for any distribution is always 0," while the maximum value is dependent on the number of industrial categories utilized (Labovitz and Gibbs 1964, p. 6). "In other words, the D value . . . reflects the degree to which persons are evenly distributed throughout a given industry structure" (Gibbs and Browning 1966, p. 88).

The industrial categories utilized were those enumerated by the U.S. Bureau of the Census (1963–64, table 75). A measure of the degree of industrial diversification based on the number of individuals employed in each industrial category was computed for each urban place, and it was this measure which was used as a correlate for the division of labor.[4]

Population size and physical density.—As mentioned above, Durkheim suggested that among the conditions accompanying the rise in the division of labor and the general trend toward organic solidarity were an increasing population size and an increasing physical density of the society. In fact, both population size and physical density were considered prerequisite to the development of a complex society based on organic rather than mechanical solidarity. Commenting on the relation between size, density, and deviance, Durkheim stated, "Insofar as a society is extended and concentrated, it envelops the individual less, and, consequently, cannot as well restrain the divergent tendencies coming up" (1964, p. 297).

In this study, population size was simply defined as the number of individuals inhabiting a particular community, that is, making it their usual place of abode. Physical density of a community refers to the population per square mile of land area (U.S. Bureau of the Census 1961).

Social density.—While the conditions of increasing size and physical density are correlates of a diminishing collective conscience and increasing

[4] The measure of the division of labor may also be based on occupations, but which of the two measures is the "better" indicator of Durkheim's concept is unknown. As Gibbs and Martin point out, "The distinction between occupation and industry in the analysis of the degree of the division of labor has evidently not been determined, conceptually or empirically" (1962, p. 669).

deviance, there is no necessary relation between them. In order for physical density to have an effect on the division of labor or the incidence of deviance, it must be accompanied by an increased social density. Durkheim suggested that the lack of means of transportation and communication demonstrates the exclusivity of the community and larger society and thus is an indication of mechanical solidarity (1964, p. 291). Paralleling Durkheim's thesis, Hawley has argued that in order for an organization to "progress with population growth beyond the limits set by a few hundred there must be an increase in the social density or, specifically, an increase in the frequency and range of interhuman contacts." This conceptualization of interaction is crucial to the definition of social density For as Hawley went on to note, social density "can be achieved only through the facilitation of movement. The term 'movement' is used here in a broad sense to include all forms of transportation through space, whether of individuals, materials or ideas as such" (1950, pp. 199–200).

Without this increase in social density, an increase in physical density will have little or no effect on the division of labor or the other processes and conditions of functional integration. The degree of transportative and communicative efficiency, then, can be used as an index of social density, and the extent of their presence can demonstrate the extent of interaction within the community as well as the degree to which the community approaches the polar collective type. Following these suggestions, social or dynamic density was operationalized as the proportion of a community's labor force employed in the following industries: railway and railway express, trucking service, other transportation, and communication.[5]

Community.—Communities were defined as those political, urban places composed of 25,000 or more inhabitants. This included incorporated and unincorporated places as well as "the towns, townships and counties classified as urban" (*Census Summary 1960*, p. xxix). In 1960 there were 765 such areas, but complete criminal statistics were available for only 691 of

[5] This operational definition was conceived and first utilized by Clemente (1969) Land, on the other hand, concluded that there was "no satisfactory measures of the efficiency of the technology of communication and transportation" and relied on the rationale of the *interchangeability of indices* to derive the measure "percentage of population in urban places" as an epistemic correlate for dynamic density (1970, p. 274). It seems that, in this case, Land is guilty of forcing his theory to fit the data for, in fact, the referent of his definition might better be conceptualized as a consequence of dynamic density rather than a measure of it. It is argued, therefore, that the operational definition utilized in this study is a more valid measure of Durkheim's original construct of dynamic density than that utilized by Land. Commenting on the difficulty of operationally measuring this construct, however, Browning and Gibbs concluded, "if 'social or moral' density is taken to be the independent variable, Durkheim's theory is untestable, because it is difficult to imagine a method of measurement, let alone securing the requisite data" (1971, p. 240).

them. Nine areas were eliminated because of incomplete census data. Thus the study population consisted of those 682 communities submitting complete criminal statistics to the Federal Bureau of Investigation and for which there was census data available for all the indices required by the research propositions.[6]

DATA SOURCE

The 1960 *U.S. Census* and the 1960 *Uniform Crime Reports* constituted the two sources of empirical data utilized in this study. There are many apparent limitations in the use of such secondary sources, among which are the problems of urban definition and the impossibility of assessing the adequacy of the methodological procedures used in the original data collection. These limitations as well as numerous problems with definitions of terms and categories make the *Uniform Crime Reports* data especially difficult to interpret.[7]

PROCEDURES

Pearson product-moment correlations were used to test the hypotheses, and a multiple-partial correlational analysis was performed. The division of labor was entered into the regression equation first, and the remaining independent variables were added in the order of their relative contribution to the reduction in the error sum of squares. This allowed for the determination of the percentage of the variance in the criterion they jointly accounted for, over and above that accounted for by the division of labor According to the Durkheimian model, the effect of the three causally antecedent variables should cancel out when variation in the division of labor is controlled. To the extent that they do not, their effects on the criterion are direct, and Durkheim's model may be modified accordingly. This procedure, coupled with the correlations of the independent variables with the criterion, partialled on the division of labor, allowed for the empirical testing of Durkheim's model.

[6] It might be argued that SMSAs would prove equally useful as the operational unit of analysis. There are, however, important theoretical and methodological objections to the use of SMSAs which are not encountered—to the same degree at least—with urban places. One effect of using SMSAs would be to eliminate many urban places with populations under 50,000, which would serve to truncate the variance and explanatory capacity of the independent variables. Whereas the narrow range of the variable "social density" is noted as a problem in a later part of this paper, the use of SMSAs could only augment the problem. Finally, for 1960 there are crime data available for only 184 of the 212 SMSAs, exacerbating the problem of truncated variance.

[7] For additional comments on the adequacy of the data reported in the *Uniform Crime Reports*, see Black 1970; Beattie 1955; Cressey 1957; and Wolfgang 1963.

In the case of the independent variables "population per square mile" and "population size," a logarithmic transformation of the original indices was used in computing the correlations. The rationale for using logs was based on an expected wide range of values for both physical density and population size and their anticipated extremes which were expected to produce less effect on the dependent variable.

RESULTS AND ANALYSIS

The first hypothesis—predicting a direct relation between the measure of industrial diversification (division of labor) of communities and the number of known serious crimes per 10,000 inhabitants—was modestly supported with a zero-order correlation (r) of .21 for property crimes and .10 for the personal crime rate (see table 1). Although the expected relationship was supported, it manifested a relatively weak association. A factor which almost certainly contributed to the low degree of association was the examination of only those cities with populations of 25,000 or larger. Durkheim set no such limits in his theoretical scheme, and certainly if smaller urban units were used it is likely that the measures which showed so little variability in this study might exhibit considerably more range. Certainly, smaller urban units would have lower measures of industrial diversification, and this alone would increase the range of the measure and thereby possibly increase the predictive accuracy of the variable. Unfortunately, the inclusion of smaller urban places was not possible, since criminal statistics were available only for those communities over 25,000.

TABLE 1

Zero-Order Product Moment Correlations for All Variables for the United States, 1960

Variables	X_1	X_2	X_3	X_4	Y_1	Y_2
Industrial diversification (X_1)	...	17	.00	.37	.21	.10
Population size (X_2)	.	..	26	13	35	44
Population per sq. mile (X_3)	.		.	08	.18	13
Proportion in transportation and communication (X_4)	...	.	...	...	.05	11
Property crime rate (Y_1)	...	.	..	...	.	60
Violent crime rate (Y_2)	. .	...	..	...	..	..

The remaining hypotheses predicted direct relationships between the independent variables and the intervening variable, industrial diversifica-

tion, and by extension indirect relationships with the criterion. With the exception of the association between physical density and diversification, the relationships are all in the predicted direction, giving general support to *the secondary hypotheses.*

Table 2 presents a summary of the regression analysis for each criterion. The magnitudes of the standardized regression coefficients indicate how much change in the dependent variables is produced by a standard change

TABLE 2

STANDARDIZED REGRESSION COEFFICIENTS AND CORRELATIONS OF NUMBER OF KNOWN SERIOUS PROPERTY CRIMES (Y_1) AND PERSONAL CRIMES (Y_2) AND PREDICTOR VARIABLES FOR THE UNITED STATES, 1960

	REGRESSION COEFFICIENTS						
	Industrial Diversification (X_1) (B)	Population Size (X_2) (B)	Population per Square Mile (X_3) (B)	Proportion in Transportation and Communication (X_4) (B)	R	R^2	MULTIPLE-PARTIAL COEFFICIENT*
Y_1	1724	.3073	.1019	— 0573	.40	16	1250
Y_2 . .	0131	4245	0165	0486	.44	.19	.1818

* This is the multiple correlation between the dependent variable and the independent variables controlling on the intervening variable, or r_i^2 (234) 1

in each predictor, controlling on the other predictor variables. With the exception of Y_1 regressed on X_4, all the coefficients are positive and in the *predicted direction. In only three instances, however, does it seem that an* independent variable exerts a substantial impact on the relative crime rates, and it is obvious that population size (X_2) is the better predictor of either criterion. The effect of all other variables on Y_2 is negligible, but for Y_1, both X_1 and X_3 exert some impact although X_2 still has the greatest influence Thus population size is clearly the strongest predictor of either criterion in this model.

The multiple correlation coefficients provide a measure of the degree to which the criterion can best be predicted from the sum effects of all independent variables acting together. For the United States, multiple correlations of .32 and .44 were obtained, indicating a moderate relationship between property and personal crime rates and the combined effect of the predictor variables. A large part of the relationship, however, is explained by the single factor of population size. The coefficient of determination indicates that at most only 16% and 19% of the total variation in property and personal crime rates can be explained by the combined action of the independent variables.

While these figures allow only a moderate degree of prediction of the dependent variable, it must be emphasized that a linear model is being assumed and that *any* degree of prediction using population parameters is theoretically significant. When the additional limitations of random and nonrandom measurement error and limitation of city size to 25,000 and over are considered, the multiple correlations do not seem to lack substantive significance.

In order to examine the relative contribution of each variable to the reduction of the residual variance, industrial diversification was entered into the regression equation first with the remaining predictor variables allowed to "float." This procedure allows us to determine if in fact the antecedent variables actually operate "through" the intervening variable, as Durkheim postulated (see fig. 1). The implication of this procedure is that when variation in the division of labor is controlled, the effect of the three causally prior variables will cancel out. To the extent that they do not, direct effects may be attributed to the antecedent variables and Durkheim's model modified accordingly. Industrial diversification accounted for less than half of the variance explained regardless of which criterion was utilized. Of greater importance, however, is the multiple-partial coefficient which represents the proportion of the variation explained by the antecedent variables over and above that explained by the intervening variable, industrial diversification. As is readily apparent in table 2, the antecedent variables jointly explain most of the variance in the property and personal crime rates. This indicates that there is a direct association between the independent variables and the criterion regardless of the division of labor. In order to further clarify these relationships, the correlations between each independent variable and each criterion were partialled on industrial diversification. The partial correlations vary only slightly from the zero-order correlations, providing additional support for the argument that direct effects exist between the independent and dependent variables.[8]

It can be concluded, then, that industrial diversification does not operate as an effective intervening variable, as postulated in the Durkheimian model. It is apparent that population size is more closely related to crime rates than any other variable.[9] This conclusion casts additional

[8] These relationships were also examined within each of the nine major regions of the United States. The multiple R ranged from .44 to .67 with Y_1 dependent and from .27 to .72 with Y_2 dependent. The multiple-partial coefficients for each region, however, were similar to those reported in table 2. That is to say, independent variables accounted for most of the variance explained.

[9] These observed associations between population size and crime rates imply that as communities experience an increase in number of inhabitants, they may also experience an increase in the rate of crimes per number of inhabitants as well as in the absolute number of crimes. This may have important consequences for a community,

doubt on the Durkheimian model, for although he recognized size as an important variable his emphasis was on density, both physical and social. Increased size was seen as necessary but not sufficient for the advancement of functional differentiation.

On the other hand, if we alter Durkheim's model and think of the division of labor as sharing the same time realm as the other independent variables, rather than as intervening between them and deviance, the model has somewhat more utility. Even then, however, the sum effects of all the independent variables in predicting the dependent variables was only moderately successful.

SUMMARY AND CONCLUSIONS

The primary hypothesis—that deviance or crime increases concomitant with an increasing division of labor—was substantiated, albeit at only a low level of predictive efficiency. Of more importance, the Durkheimian model, postulating the division of labor as an intervening variable between population size and physical and social density and the dependent variable, crime, has been largely rebutted.

Perhaps the best test of Durkheim's conceptual scheme was provided by the regression analysis which allowed determination of the total variance which was explained by the united action of the predictor variables, and by the calculation of a multiple-partial coefficient, which demonstrated that the antecedent variables had direct effects on the criterion. These findings imply that Durkheim's model must be modified to include independent effects of population size, physical and social density on the criterion, deviance.

Thus Durkheim's model can neither be said to be verified nor can it be discarded, for when all the variables are allowed to operate jointly, they have differential but direct impact on the dependent variables. These findings, then, raise the question of why the model did not allow for greater prediction than was attained.

Among the usual explanations for the failure of a model to attain perfect prediction are measurement error, an erroneous or incomplete conceptual model, and an inappropriate test model. The latter reason requires some elaboration in explaining the failure of this model to approach unity. As mentioned earlier, the coefficient of determination (R^2) is affected by the heterogeneity of the universe examined. The necessity of ignoring urban units under 25,000, therefore, served to truncate the variance and the explanatory capacity of the independent variables. Thus it must be empha-

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for it highlights the fact that a community can expect its crime rate to vary directly with the size of its populace.

sized that this test was limited to a particular part of and not the entire universe in question. Clearly, Durkheim was concerned with ecological communities of all sizes and set no size limits in his theoretical scheme.

The single most important limitation of this study, however, involves a somewhat different conceptualization of the evolutionary process wherein a community progresses toward functional integration and an increased division of labor. Durkheim, of course, was concerned with the evolution and organizational development of individual communities through time, whereas this study examined a universe of several cities—of varying size, density, and diversity—at one point in time and assumed this to be a valid surrogate for the examination of a single city at several points in time. This, of course, is an important limiting assumption, and to the extent it is erroneous it may at least in part be responsible for the failure of the model to be wholly verified. However, until sufficient longitudinal material is available to establish single city trends the present approach seems to provide the only alternative for examining the propositions in question.

These limitations notwithstanding, an inappropriate operational model is probably only partially responsible for the imperfect unity predictions Of equal importance is the possibility of an erroneous or incomplete conceptual model. Certainly the model examined here is a very simple one, assuming linear relationships between a very few variables. It may well be that several relevant variables are not included in this model and the additive effects of these unknown factors might allow a substantial reduction in the residual prediction error. On the other hand there are those who argue in favor of quite modest working theories (see Gordon 1968, p. 593; Blalock 1968, pp. 196–97).

While Durkheim's model is quite probably "incomplete," this research also raises a question of the validity of its emphasis on density. As noted earlier, Durkheim posited that population size was of consequence to deviance only insofar as the density (physical and social) of the community increased. And in addition, he argued that size and density affected deviance through the division of labor. Yet the findings presented here demonstrate that size is more closely related to crime rates than either density or industrial diversification.

There is a substantial amount of literature which suggests that population size is somehow directly related to crime rates. This relationship is usually couched in terms of an urbanism/urbanization framework in which urbanization is construed as the processual increase in the proportion of the population residing in urban areas. While there is little doubt that most crime rates generally increase with city size, it is the explanation of this association that is of greatest interest. The criminological literature is replete with "explanations," most of which deal with the characteristics

of the social milieu (e.g., high mobility, impersonality, normative conflict, differential opportunities for crime, etc.). In these cases, urbanization is considered primarily as an antecedent variable with the "real" explanation of deviance residing in the characteristics of "urbanism as a way of life."[10]

Other explanations of urban crime are also found throughout the literature. Some would have it that high urban crime rates are simply an artifact of differential reporting procedures between rural and urban areas. Alternately, some have attributed the higher urban rates not so much to the criminogenic characteristics of these areas as to the tendency of "deviant" persons to drift to these areas. Perceived differential deterrence may also operate to inflate urban crime rates; that is, the certainty of arrest and sanction being inversely related to population size. Whatever interpretations are utilized to "explain" the association between urban size and deviance, however, most are subsumed under the generic rubrics of urbanism and urbanization. Yet there seems to be little macrosociological research treating the obvious relationship between crime and the increasingly complex form of social organization inherent in and fundamental to urban development (see Webb and Clemente 1972).

In essence, while any or all of the above factors may account for the failure of the model to be completely verified, in reality this failure is most likely a result of all these conditions working together with varying degrees of influence.

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[10] For an excellent review of this literature, see Clinard 1964, 1968

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Structural Change and the Durkheimian Legacy: A Macrosocial Analysis of Suicide Rates[1]

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This paper presents a further specification of Durkheim's theory of suicide. A propositional theory is developed relating population growth, technological development, changes in the division of labor and social integration, and suicide rates. Results are mixed, but are generally as predicted. Strongest support results from the relationships between changes in level of technology, the division of labor, social integration, and suicide rates. Results contrary to the theory were obtained when the effects of population change were considered.

Sociology can claim few instances in which a theoretical formulation has been pursued rigorously over a period of time such that its predictive power, scope, parsimony, and logical consistency are progressively enhanced. The reasons underlying this failure are beyond the scope of the present inquiry, though they may be related to an inordinate emphasis on "unique contributions" in the contemporary sociological ethos and a corresponding lack of reward for explication and synthesis of existing theories. Be that as it may, the present effort may be viewed as an attempt to further specify one of the few sociological theories which has received continuous and constructive attention since its initial presentation, that is, Durkheim's (1951 [1897]) theory of suicide.

The overpowering impact of Durkheim's ideas on subsequent studies of suicide has been noted by a number of writers (Martin 1968). But to acknowledge the widespread recognition and acceptance of Durkheim's seminal contribution is not to say that we cannot benefit from periodic reassessment of this great work. Indeed, recent and enlightening evaluations have been made from the points of view of methodology (Selvin 1965), conceptualization (Dohrenwend 1959; Johnson 1965), and theory construction (Gibbs and Martin 1958*a*, 1964). The argument to be presented here is, like the efforts of Gibbs and Martin, an attempt to add further precision to Durkheim's exposition of the relation between social

[1] We would like to express our appreciation to Warren Breed for his helpful comments on an earlier version of this paper.

structure and the suicide rate. However, rather than begin with Durkheim's basic theme that "suicide varies inversely with the stability and durability of social relationships" as these writers have done, we shall try to enter the causal chain at an earlier point. We propose to draw from Durkheim's writings an argument that identifies some of the major components of change in social structures which result in variations in the degree of social integration—in the stability and durability of social relationships. The resulting theory will be tested utilizing data for a series of selected countries.

DERIVATION OF THE THEORY

Examination of an argument presented in *Suicide*, considered in the light of *The Division of Labor in Society* (1964 [1893]), can yield a testable theory of the relationship between the rapidity of change in social structure and suicide. In book 2 of *Division*, Durkheim attempted to explain the conditions under which mechanical solidarity gives way to organic solidarity. Population increase was held to play a large part in bringing about this transformation, although its role was a necessary rather than a sufficient cause. Segmentation breaks down in the face of increased moral density. Moral (or dynamic) density refers to an increased frequency of social interaction within a given social unit; physical density refers to the absolute number of people per unit of space. The division of labor, or degree of differentiation, increases as the rate of interaction increases. The rate of interaction (moral density) may increase with increased concentration of population, especially in cities, and with the development of more rapid and numerous means of transportation and communication, which can produce increases in effective moral density, even though no increase in size or concentration is involved. (See Schnore [1965], pp. 8–10, for a discussion of this point.) Differentiation accompanies growth provided that interaction increases concomitantly; but interaction can increase without growth if transportation and communication improve.

Several writers have pointed out that *The Division of Labor in Society* contains the seeds of all Durkheim's later work (Nisbet 1965, p. 44, Schnore 1965). Certainly he did not neglect the topic of suicide in *Division*. In chapter 1 of book 2, entitled "The Progress of the Division of Labor and Happiness," increases in suicide rates are said to be related to the progress of the division of labor. In attempting to demonstrate that happiness does not increase as a result of increased division of labor, Durkheim proposes to regard the suicide rate as a measure of average unhappiness, and contends that "suicide scarcely appears except with civilization. . . . It is rare in lower societies" (1964, p. 246). There are of course many

difficulties with this "suicide-civilization" thesis, not the least of which is the fact that there are substantial differences in the suicide rates of countries at about the same level of civilization. This thesis is not seriously considered in *Suicide*.

Durkheim does not hold that the division of labor is the direct cause of suicide. Further on, we find the following: "Is that to say that it is necessary to impute these sad results to progress itself, and to the division of labor which is its condition? This discouraging conclusion does not necessarily follow from the preceding facts. It is, on the contrary, very likely that these two orders of facts are simply concomitant. But this concomitance is sufficient to prove that progress does not greatly increase our happiness, since the latter decreases, and, in very grave proportions, at the very moment when the division of labor is developing with an energy and rapidity never known before" (1964, pp. 249–50).

There is a suggestion here that the rapidity of the development of the division of labor is connected with an increase in suicide. This might be interpreted to mean that with the degree of division of labor held constant, the suicide rate would vary directly with the rate of change in the division of labor (cf. Moore 1963, pp. 1–44).

A statement with this implication can be found in *Suicide*: "Our social organization, then, must have changed profoundly in the course of this century, to have been able to cause such a growth in the suicide rate. So grave and rapid an alteration as this must be morbid; for a society cannot change its structure so suddenly. . . . Thus, what the rising flood of voluntary death denotes is not the increasing brilliancy of our civilization, but a state of crisis and perturbation not to be prolonged with impunity" (Durkheim 1951, p. 369). It should be noticed here that Durkheim is reasoning in reverse, from "the rising flood of voluntary deaths" to profound changes in social organization. Nonetheless, the following proposition is implied: "Suicide varies directly with the rate of change in the division of labor."

This raises a crucial question. If "suicide varies inversely with the degree of integration of the social groups of which the individual forms a part" (Durkheim 1951, p. 209) and directly with the progression of the division of labor, does the division of labor weaken the integration of society? Durkheim implies that it does. There is a strong resemblance between his description of the mechanical mode of organization in *The Division of Labor* and his description of strong social integration in *Suicide*. (Compare, for example, *Division*, p. 130 and *Suicide*, p. 209.) Further (and more importantly for the present argument), there is some similarity between conditions associated with rapid increase in the division of labor and the "suicidogenic" conditions of weak integration. In *Suicide*, excessive indi-

viduality is regarded as one of the precipitating factors in egoistic suicide, and rapid development of the division of labor is held to produce excessive individuality.

Nisbet (1965) points out how Durkheim changed his original conception of *The Division of Labor* in the course of writing it: "Durkheim could see that, although the conceptual distinction between the two types of solidarity or association was a real one, the institutional stability of the second had to be deeply rooted in the continuation—in one form or another—of the first" (p. 36). Organic solidarity is built upon mechanical solidarity, rather than displacing it.

This suggests an answer to a question raised by the analysis in *Division*. Why do rapid advances in the division of labor predispose toward higher suicide rates? Put simply, this rapid increase tends to break down the mechanical basis of organization which forms the substratum for specialization and functional interdependence. In the terminology of *Suicide*, weak social integration results from such rapid advances.

It is thus possible to piece together, from *Division* and *Suicide*, a theory of the relationships among population growth and technological development, the division of labor, social integration, and suicide.

Population growth and technological development produce increased moral density, which contributes to "the progress of the division of labor." The rate at which the division of labor develops is an important variable because it influences social integration. Social integration, which "has to do . . . with the strength of the ties of individuals to society" (Gibbs and Martin 1964, p. 16), is strongest when the society is characterized by mechanical solidarity. Rapid development of the division of labor tends to disrupt the mechanical basis of organization and to weaken the ties of the individual to society. Thus, if suicide varies inversely with social integration, then it should vary directly with the rate of increase of the division of labor.

Stated in propositional form, these relationships are as follows:

Proposition I:	The higher the rate of population growth, the greater the rate of increase in the division of labor.
Proposition II:	The higher the rate of technological growth, the greater the rate of increase in the division of labor.
Proposition III:	The greater the rate of increase in the division of labor, the greater the rate of decrease in social integration.
Proposition IV:	The greater the rate of decrease in social integration, the greater the suicide rate.
Corollary I:	The higher the rate of population growth, the higher the suicide rate.
Corollary II:	The higher the rate of technological growth, the higher the suicide rate.
Corollary III:	The greater the rate of increase in the division of labor, the higher the suicide rate.

In their present form these propositions are framed in terms of rates of occurrence of specified events (e.g., "population growth," "increase in the division of labor," and "suicide" considered over a fixed interval of short duration. There is, however, an alternative form in which they might be stated, that is, in terms of rates of change in the basic measures themselves over a longer span of time. For example, Corollary I would read: "The greater the increase in the rate of population growth, the greater the increase in the suicide rate." For this latter form one might wish, were the requisite data available, to examine the relationships posited by the theory over a 20- or 30-year interval. With a shorter period of time, however, where there is little variation likely in the basic rates, differences in results from the two alternative forms of the theory will be minimal. (For a consideration of some of these issues see Micklin 1970.)

In schematic form, the theory appears as follows:

Population Growth ⟶ Division of Labor ⟶ Social Integration ⟶ Suicide.
Technological Growth ⟶ Division of Labor

In this diagram we have not specified the logical connections between social integration and suicide, but would adhere to those specified by Gibbs and Martin (1964). These are discussed below in the section on measuring social integration. It should be further noted that we have not indicated the expected relationship between population growth and technological growth. For one reason, this is not central to our theory, and for another, there are too many potentially confounding variables to be considered here. The following sections of the paper are concerned with operational measurement of the variables and an attempt to test the theory using data derived from several countries.

DATA AND METHODS

The theory has been tested using data from three kinds of units: nations, individual states within the United States, and U.S. SMSAs (Miley 1970). This paper, however, will report only one series of tests: those for countries. The measurement procedures for the variables are discussed below.

Population growth.—Population growth is the only independent variable for which direct measures are available, and these can be expressed in at least two ways: as the average annual or the percentage growth between two points in time. If the time interval is the same for every case, either of these measures is adequate; their rank order will be the same. However, census periods vary from country to country, so that average annual percentage growth was used in this case.

Technological development.—Technology involves the use of energy, and increasing levels of technology are accompanied by increasing use of nonhuman energy sources. This is the rationale for using per capita consumption of energy as the operational definition of technological development. The use of per capita energy consumption has become a fairly standard index for this variable (Saunders and Reinhart 1967; Gibbs and Browning 1966; Gibbs and Martin 1958*b*, 1962; and Marsh 1967). Consumption of energy has been expressed in several ways. Two of the most common are in terms of metric tons of coal or in terms of megawatt hours of electricity per capita. This study uses data for estimated per capita consumption of commercial sources of energy expressed in terms of metric tons of coal. The time interval used is the 10-year period, 1950–60. The rate of change in technological development for countries is operationally defined as the difference in this index between 1950 and 1960. It is not necessary to compute a formal rate of change for this measure since the time interval in each case was the same.

The division of labor.—An increased division of labor involves at least the following elements: a proliferation of specialities; an increase in the ratio of formal to informal norms; an increase in the proportion of occupations devoted to coordination (Mott 1965); and as a consequence of these, an increased probability of role conflict, which provides the theoretical link between the division of labor and the social suicide rate.

The measure of change in the division of labor which was adopted is the "index of dissimilarity." This index has been used by Duncan and Duncan to measure the spatial distance between occupational groups (1955), by Taueber and Taueber (1965) to measure the extent of residential segregation in cities; and by Gibbs (1968) to measure change in industry structure. The computation of this measure is shown in the hypothetical example in table 1. The formula is: $D = A - B/2$. The index of dissimilarity (D) "is then one-half the sum of the absolute values of

TABLE 1

COMPUTATION OF INDEX OF DISSIMILARITY

Area	Occupations A (%)	Occupations B (%)	Difference (%)
1	10	15	5
2	20	15	5
3	40	25	15
4	30	45	15
Total	...	...	40

SOURCE.—Duncan and Duncan 1955.

the differences between the respective distributions, taken area by area" (Duncan and Duncan 1955, p. 494). In this example, the index is 20 (40/2). This is interpreted as a measure of displacement: 20% of the workers in either occupation would have to move to a different area in order to make the distributions identical.

Taueber and Taueber (1965) have interpreted the measure in essentially the same way, calling it an "index of segregation." Here it is used to show the proportion of Negroes or whites who would have to move to another tract to achieve a racial balance.

Gibbs (1968) has utilized this measure as an index of change in industry structure by substituting occupational categories for areas and the distribution of workers in these categories at two points in time for occupations. The result is an index of change in occupational distribution between two points in time. The computation of the measure for one country is shown in table 2. This measure should be an adequate index of change in

TABLE 2

INDUSTRY COMPOSITION OF ECONOMICALLY ACTIVE MALES, 15 YEARS OF AGE AND OVER, AND INDEXES OF DISSIMILARITY, VENEZUELA, 1950 AND 1961

	1950			1961	
INTERNATIONAL NOMENCLATURE	Number (1)	Proportion (X) (2)	|X − Y| (3)	Number (4)	Proportion (Y) (5)
0 Agriculture, forestry, hunting and fishing	704,704	0.413	0.089	773,650	0.324
1 Mining and quarrying	44,509	0.026	0.006	46,675	0.020
2-3 Manufacturing	172,493	0.101	0.023	294,975	0.124
4 Construction	91,104	0.053	0.001	128,125	0.054
5 Electricity, gas, water, and sanitary services	5,219	0.003	0.008	25,425	0.011
6 Commerce	149,678	0.088	0.040	304,375	0.128
7 Transport, storage, and communication	52,329	0.031	0.014	106,575	0.045
8 Services	342,114	0.200	0.040	571,700	0.240
9 Activities not adequately described	144,171	0.084	0.028	134,725	0.056
Total	1,706,171	0.999	0.248	2,386,225	1.002

SOURCE.—*Yearbook of Labour Statistics*, vols. for 1955 and 1965. The formula for the index of dissimilarity is $(\Sigma|X - Y|)/2$. For the data in this table this amounts to .248/2 = .124.

the division of labor. It utilizes the categories contained in the International Standard Industry Classification (ISIC), though these may be too gross since they do not reveal the emergence of subindustries. Moreover, the measure may reveal the amount of change but not its direction. However, Gibbs's data indicate that "substantial changes in industry structure tend

to take the same direction in all countries" (1968, p. 42). This change is presumably in the direction of more complexity. If this is so, the differences among countries are in the amount of change rather than its direction, and this would mean that the measure is satisfactory in terms of change in industry structure.[2] Whether it is an equally satisfactory index of the various aspects of the division of labor mentioned above is a different question and one to which we unfortunately have no answer.

Social integration.—Social integration is the most difficult variable to deal with. Landecker (1951) and Gillin (1948), among others, have suggested that several different variables are involved, and each suggests four different types of integration. None of these appears to be measurable, with the exception of Landecker's fourth type, "functional" integration, which refers to interdependence through the division of labor. In the present study this is treated as an independent variable which influences integration rather than as a type of integration.

The decision was made to use Gibbs and Martin's (1964) measure of status integration as an index of social integration. The reasoning behind

[2] An alternative measure of the division of labor was computed after the initial analysis was completed. This measure was one proposed by Gibbs and Martin (1962), for which the formula is $1 - [\Sigma X^2/(\Sigma X)^2]$, where X is the number of people in each of the nine industry categories of the ISIC. This measure was computed for 1950 and 1960 for each country, and then the 1950 measure was divided into that for 1960 ($D2/D1$). The results were compared with the index of differentiation (I.D.) scores used in the initial analysis, with results which were entirely unanticipated. First, the rank-order correlation (ρ) between the two measures was almost nonexistent (—.04). Second, the correlation between the $D2/D1$ measure and the 1960 suicide rates is also nil (—.03). Third, the correlations between this measure and the other variables are in the opposite direction from our predictions and the findings of our initial analysis. The correlation between the $D2/D1$ measure and technological development is —.19, while that using the I.D. is .36. That between the $D2/D1$ measure and the status integration measure for 1960 is .46, while that using the I.D. is —.26. (These correlations are not strictly comparable, since one is ρ while the other is r; nonetheless the differences in direction are significant.) Comparing specific examples of the two measures is instructive. For example, Guatemala ranked twenty-fourth on the I.D., but third on the $D2/D1$ measure. Looking at the data for this country, the I.D. ranking makes more intuitive sense, since there was no change in most of the industry categories and small amounts of change in the others. What looks like a small amount of change in the raw data—and the I.D.—turns out to be a large amount when measured by $D2/D1$. Japan, however, ranked first on both measures. In general we find that when the rankings are different, the I.D. rankings jibe more closely with the data and with what we know about the countries in question. However, we have no theoretical reason to believe either measure to be clearly superior to the other. A basic difference between the two is that the I.D. reflects the magnitude of change but not its direction, while the $D2/D1$ measure does reflect the direction of change. Apparently this is related to the lack of correlation between the two measures, since eight of the 24 countries had $D2/D1$ values below unity, which presumably indicates change toward less division of labor. With respect to the correlation with suicide, it may be that the fact of change is more important than the direction of change in the determination of suicide rates. However, we have no substantial explanation for these inconsistent findings.

this decision is as follows. Suicide is related to social integration (Durkheim 1951). Integration "has to do . . . with the strength of ties of individuals to society" (Gibbs and Martin 1964, p. 16). The ties of the individual to society are the strongest when the probability of predicting all of the individual's statuses by knowing one of them is greatest. This probability is highest when the society is characterized by mechanical solidarity and thus has a minimal division of labor. Conversely, it is lowest with an elaborate division of labor.

The status integration measure is essentially an index of the predictability of status occupancy. The measure is highest when all individuals who occupy status X also occupy status Y—that is, when the occupancy of status Y is completely predictable from the occupancy of status X.

The computation of the status integration index involves summing the squared proportions for each column within a tabular cross-classification of statuses (X^2). Separate measures for each column are simply the X^2s for those columns. An example of the computation of this measure is given in table 3.

TABLE 3

INTEGRATION OF LABOR-FORCE STATUS WITH SEX IN CHILE, 1960 AND VENEZUELA, 1961 FOR PERSONS 15 YEARS OF AGE AND OVER

Labor-Force Status		Chile		Venezuela	
		Males	Females	Males	Females
Economically active (in labor force)	$X =$	0.851	0.227	0.895	0.211
	$X =$	0 149	0.773	0.105	0.789
Not economically active (not in labor force)	$\Sigma X =$	1 000	1 000	1.000	1.000
	$\Sigma X^2 =$	0.746	0.650	0.812	0.666
	$\Sigma\Sigma X^2 =$	1.396		1.478	

NOTE.—The model for this table may be found in Gibbs and Martin (1964), table 41, p 122. The source of data for these countries is the *Demographic Yearbook* (1964), table 8, p. 190.

Suicide.—Suicide rates for countries are published by the World Health Organization and are also given in some issues of the *Demographic Yearbook*. The specific sources of suicide rates are cited in table 4. The adequacy of official suicide statistics has been questioned by Douglas (1967). The argument presented there is neither refutable nor demonstrable, since it is based on plausible conjecture rather than evidence. Actually, there is very little evidence bearing on this question. As a result, an assessment of the reliability of suicide rates in general is not possible; only particular rates may be assessed, and these may or may not be found adequate. But the adequacy of the suicide rate for a particular country or city at some

point in time has little bearing on the adequacy of any other rate at any other point in time. Likewise, the possibility that officials in charge of classifying deaths may have quite different definitions of suicide, a point which Douglas belabors, has a completely unknown influence on the suicide rate of any population. The position we have taken is that the question is still open. If a theory of the present type is to be tested, official suicide statistics must be used. There is no alternative.

Test of hypotheses.—Hypotheses are tested primarily using the Pearsonian product-moment coefficient of correlation (r). This statistic has the advantage of allowing the computation of multiple and partial correlations, for example, the relationship between population growth and the division of labor, controlling for level of technological development, which would provide a test of Propositions I and II in combination. The disadvantages of the use of this measure in the present context are: (1) r assumes a linear relationship, whereas there may be curvilinear relationships between the variables in the theory; (2) tests of significance for this measure assume random sampling, not utilized in this study, since availability of data was the main criterion used in the selection of the sample. In a few instances we have utilized Spearman's coefficient of rank-order correlation (ρ). For discussions of both these statistics see Blalock (1960).

INTERNATIONAL COMPARISONS: SELECTED COUNTRIES

The theory was tested using the data shown in table 4. For these data the matrix of zero-order correlations is shown in table 5. The status integration measures used in these tests are measures of the integration of labor-force status with sex. The computation of this measure is shown in table 3 above. The figures from 1950 were drawn from Gibbs and Martin (1964), while we computed the figures for 1960 based on the information in table 6. For purposes of comparison we began with the countries used by Gibbs and Martin (1964) in testing the status integration theory at that level. Data limitations reduced the original list of 32 countries to the 24 shown in table 4.

Proposition I predicts a direct relationship between population growth and the division of labor. For the indicators used here, the correlation (r) is —.16, which is a reversal of the prediction. This reversal is discussed below. It should be noted here, however, that for the present series of tests, there is practically no relationship between population growth and technological development ($r = .02$). We had expected these variables to be directly related, although this was not stated in the theory. It was felt that their combined influence would be responsible for change in the division of labor. This is a direct extension of Durkheim's argument in *The*

TABLE 4

Average Annual Population Change, Change in per Capita Consumption of Energy, Index of Dissimilarity Scores Based on Labor-Force Distributions, Integration of Labor-Force Status with Sex, and Suicide Rates

Countries	Annual Average Population Change, 1950–60	Change in per Capita Energy Consumption, 1950–60	Index of Dissimilarity	Integration of Sex and Labor-Force Status		Suicide	
				1950	1960	1950	1960
Mexico	3.1	0.32	.075	1.553	1.747	1.0	1.8
Guatemala	3.2	0 03	.035	1.664	1.639	1.5	3.0
Ireland	−0.5	0.74	.060	1.340	1.410	2.5	2.7
Costa Rica	3.8	−0 02	.073	1.623	1.544	2.6	2.4
Greece	1 0	0.35	066	1 493	1.230	3.6	3.8
El Salvador	2.8	0.04	.054	1.605	1.550	4.1	11.3
Chile	2.5	0 08	.046	1.394	1.396	4.4	7.6
Venezuela	3.7	1.85	.124	1 521	1.478	4 7	6.4
Netherlands	1 3	0 73	.117	1.355	1.362	7.0	6.8
Norway	0.9	−1 63	068	1 375	1.350	7.2	7.5
Canada	2 7	−0 81	.097	1.347	1.240	7.4	7.5
Israel	5 3	0 44	061	1 361	1.244	8.6	7.0
Australia	2 3	0 80	107	1 404	1.344	9.7	11.3
Portugal	0 5	0.12	.057	1.442	1.554	10.1	8.8
New Zealand	2.2	−0 30	.051	1.338	1 344	10 3	9.7
United States	1 8	0 31	081	1 255	1.210	10 9	10.7
France	0.9	0 39	077	1.255	1.202	12.4	16.4
Belgium	0.6	0.47	143	1.317	1.256	14 7	14.2
Sweden	0.6	0 27	075	1.336	1.224	15.6	17.6
Finland	1.0	0 48	.116	1.284	1.234	16.4	20.6
Japan	1 1	0.39	.168	1.224	1.248	17 9	23.2
Austria	0 2	0 62	.095	1 236	1.176	23 4	23 7
Denmark	0 7	0 74	076	1.295	1.284	23 9	20.8
West Germany	0.9	1.12	.118	1.251	1.238	17.9	18.8

Source.—Population data from Davis (1969), table D, pp 139–60. Energy consumption figures from U.N. *Statistical Yearbook* (1953, table 127; 1964, table 131); 1950 status integration measures from Gibbs and Martin (1964), table 42, p. 124. Status integration measures for 1960 computed by us from labor-force data in the *Demographic Yearbook* (1964), table 8. These data are shown in table 6. Suicide rates circa 1960 from *Epidemiological and Vital Statistics Report* (1962), table 2. Rates are three-year averages for 1958, 1959, and 1960. The 1960 rates for Mexico and Chile are from the *Demographic Yearbook* (1966), table 20, for 1960 only. Index of dissimilarity scores were computed from labor-force composition data in *Yearbook of Labour Statistics* (1955, 1965, 1966). See table 2 for the computation of this measure.

Division of Labor, but the present results are inconsistent with that position.

Turning to Proposition II, which predicts a direct relationship between technological change and change in the division of labor, we find that for these two variables the correlation (r) is .36, which is in the predicted direction.

Proposition III predicts an inverse relationship between change in the division of labor and change in status integration. The correlation between the indexes of dissimilarity for the various countries and the 1950 status

TABLE 5

MATRIX OF CORRELATIONS BETWEEN SEVEN VARIABLES FOR 24 COUNTRIES

	Population Growth	Technological Development	Integration of Sex and Labor-Force Status		Suicide		Change in Division of Labor
			1950	1960	1950	1960	
Mean	2.27	1.94	1.38	1 35	9.90	10.98	0.08
SD	1 35	0.63	0.12	0.15	6.52	6.71	0.03
Correlation with:							
Population growth	1.0000	...	..	...	...	...	...
Technological development	—.0210	1.0000	..	...	...	...	...
Integration of sex and labor-force status, 1950	.5672	— 0999	1 0000	...	...	...	...
Integration of sex and labor-force status, 1960	.4048	—.0575	.8345	1 0000	...	...	...
Suicide, 1950	—.5090	.2154	— 7658	—.6749	1 0000	...	...
Suicide, 1960	— 4872	.2170	— 7144	—.6354	9416	1.0000	...
Change in division of labor	—.1612	.3611	— 4440	— 2635	.4102	.4414	1.0000

TABLE 6

ECONOMICALLY ACTIVE OVER 15, BY SEX, FOR 24 COUNTRIES CIRCA 1960

Country	Year	Lower Age	Male	Female
Mexico	1960	15	92.4	19.7
Ireland	1961	15	84.2	28.9
Costa Rica	1963	15	90.8	17.5
Greece	1961	15	80.6	35.5
El Salvador	1961	15	92.4	19.2
Chile	1960	15	85.1	22.7
Venezuela	1961	15	89.5	21.1
Netherlands	1960	15	82.6	22.6
Norway	1960	15	82.6	23.8
Canada	1961	15	78.1	29.7
Israel	1961	14	76.7	27.5
Australia	1961	15	85 7	28.9
Portugal	1960	15	91 0	17.0
New Zealand	1956	15	83.7	26.0
United States	1960	15	78.9	35.1
France	1962	15	78.6	36.2
Belgium	1961	15	76.0	25.5
Sweden	1960	15	78.6	32 7
Finland	1960	15	84.2	48.5
Japan	1955	15	85.2	50.6
West Germany	1961	15	83.3	41 1
Austria	1961	14	79.1	44.5
Denmark	1960	15	85 2	36.5
Guatemala	1964	15	92.8	13.6

SOURCE —*Demographic Yearbook* (1964) table 8, p. 190

integration measures is —.44. The correlation between the indexes of dissimilarity and the 1960 status integration measure is —.26. These relationships are both in the predicted direction and indicate that change in the division of labor is negatively associated with the measure of status integration. However, the proposition predicts that the higher the rate of increase in the division of labor, the greater will be the decline in social integration. The differences in the status integration measures were for the most part quite small. For this reason, no change rates were computed in the initial analysis. A later analysis, however, has shown that there is essentially no relationship ($\rho = -.06$) between change in the integration of labor-force status and sex and the indexes of dissimilarity.

The prediction from Proposition IV is that "the greater the decrease in social integration, the higher the suicide rate." For this period the mean status integration declined from 1.38 to 1.35, while the mean suicide rate increased from 9.90 to 10.98. The correlation between the status integration (S.I.) scores and suicide rates for 1950 and 1960 are fairly strong and provide additional support for Gibbs and Martin's status integration theory. The correlation for the 1950 figures is —.77, while that for the 1960 figures is —.64.

The procedure used to test the proposition was simply to indicate which countries showed an increase in status integration and which showed a decrease. Increases or decreases in the suicide rate for the same time period were also noted. Each case in which the status integration measure declined while the suicide rate increased, or vice versa, was regarded as supporting the proposition, while each case in which an increase in status integration was accompanied by an increase in suicide, or vice versa, was regarded as not supporting the proposition. Fifteen cases supported the proposition, while nine did not. Of the 15 supporting cases, Australia, Austria, Canada, El Salvador, Finland, France, Greece, Guatemala, Norway, Sweden, Venezuela, and West Germany showed a decrease in status integration and an increase in suicide, while the Netherlands, New Zealand, and Portugal showed an increase in status integration and a decrease in suicide. Of the negative cases, Chile, Ireland, Japan, and Mexico showed an increase in both variables, while Belgium, Costa Rica, Denmark, Israel, and the United States decreased on both variables.

Turning to the corollaries, we find that the correlation designed to test Corollary I is in the opposite direction from that predicted. The prediction is for a direct relationship between rates of population growth and suicide rates. However, the correlation between population growth and suicide for 1960 is —.49, a direct reversal of the prediction.

The tests of Corollary II are in the predicted direction. The correlation (r) between technological change and suicide for 1960 is .22. A multiple coefficient of correlation combining the first two corollaries was computed, treating the 1960 suicide rates as the dependent variable and population growth and technological development as the independent variables. The resulting R is .54.

With respect to Corollary III, which predicts a positive relationship between the rate of increase in the division of labor and suicide rates, the present test resulted in a correlation of .44 between the index of differentiation scores and the 1960 suicide rates. This is in the predicted direction, and appears to give some support to this corollary.

SUMMARY

The testing of this theory was launched by a reversal of the prediction of Proposition I for a positive correlation between population change and change in the division of labor.

Proposition II, predicting a direct relationship between change in technological development and the division of labor, was supported at the level of $r = .36$.

With respect to Proposition III, a rank-order correlation showed essentially no relationship between change in the division of labor and change

in status integration, although there were positive *r*'s between change in the division of labor and level of status integration.

The prediction from Proposition IV was an inverse relationship between change in social integration and suicide. The correlations between status integration and suicide for 1950 and 1960 were quite strong, but did not provide a direct test of the proposition. To provide such a test, the direction of change in status integration and suicide was noted for each country. Each case in which these variables changed in the opposite direction was regarded as supporting the proposition. There were 15 such cases among the 24 countries.

The prediction of Corollary I for a direct relationship between population change and suicide was reversed. The prediction of Corollary II was supported by a low positive correlation between technological development and suicide. Corollary III was also supported by positive *r*'s between change in the division of labor and suicide rates.

The general picture presented by these tests is mixed. Two of the propositions are supported (II and IV), one is reversed (I), and one is partially reversed (III). Of the corollaries, one is reversed (I) while two are supported (II and III).

It is interesting that both predictions (Proposition I and Corollary I) containing population change as the independent variable are reversed. For Corollary I this may be related to the unexpected positive relationship between population change and status integration.

DISCUSSION

One important difference between the present theory and the Gibbs-Martin status integration theory is that while both are based on causal chains, the Gibbs-Martin theory deduces only one testable proposition from a series of untestable ones, whereas the present theory is testable at every point. This, however, is not an unmixed blessing.

If the variables of the theory operate in the posited causal sequence, and if there are uncontrolled variables operating at every point in the chain, then the correlations between adjacent variables (e.g., technological development and the division of labor) should be higher than those between nonadjacent variables (e.g., technological development and suicide). However, the highest correlation in the predicted direction is between two nonadjacent variables—change in the division of labor and suicide. Thus with respect to this logical expectation, our results are ambiguous.

An additional implication of this form of theorizing is that there are actually several theories contained in the causal chain: the theory of suicide is not the only output from the present set of propositions and corollaries. Durkheim's theory of the development of the division of labor

(or more accurately, our interpretation of it) is contained in this chain and is tested by the observed correlations. Additionally, there is a theory of social integration within the theory of suicide. The only variables which are not dependent in any of the propositions and corollaries are population growth and technological development.

It should be noted that all the possible derivations are not contained in the formal statement of the theory. For example, the corollary "the higher the rate of population growth the greater the decrease in social integration" may be logically derived from Propositions I and III. It was thought more advisable to limit the derivations to the three corollaries, all of which have suicide rates as their dependent variable, and otherwise to view the relationships among the variables in terms of a sequential chain in which each variable is influenced by the one immediately before.

In conclusion, the present paper has attempted to deduce from the writings of Durkheim a propositional theory of variation in suicide rates and to put the theory to a series of tests. The theory has conceived rates of change in the relevant variables to be crucial in the explanation of variation in rates. The tests were largely positive, though some were not direct tests of the theory.

As a final note, it should be apparent that the present testing procedure is not the only way of testing the theory. Many other procedures are possible, some no doubt superior to the one used. It is our hope that the results of the present tests will stimulate others.

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Commentary and Debate

COMMENTARY ON RACIALLY CHANGING NEIGHBORHOODS

Observers have long believed that whites tend to flee when blacks move into their neighborhoods. Harvey Molotch (1969), followed by Avery M. Guest and James J. Zuiches (1971), presented a refreshing alternative interpretation of the flight of whites from the inner city, namely, that these areas generate a good deal of outmigration for other reasons.

Molotch deserves ample credit for opening this area of inquiry with his case studies of Chicago's South Shore and Rogers Park. Guest and Zuiches are to be commended for their application of statistical controls to a sample of census tracts in Cleveland, Ohio. Unfortunately, they committed the very sin for which they criticized Molotch. That is, they biased their results in favor of the interpretation that white outmigration could be attributed to nonracial causes by failing to consider black movement into white areas as an independent variable until after they had allowed all other variables to have maximum effect. If anything, they should have biased the analysis in favor of the "invasion" hypothesis, since, as critics, the burden of proof was on them.

Unwittingly, the authors also made several serious errors of causal specification. First, their regression model made the unwise specification that high population density and more industrial land use make people remain in a neighborhood. Second, they specified in their model that the decline in the percentage white collar in inner-city communities caused higher mobility from these neighborhoods, when indeed the causation ought to work the other way around, since our theory tells us that lower-status people follow higher-status people in ecological succession. Third, their other "control variables" are the very factors which caused racial succession by engendering high mobility rates in the past. Removing the effects of its causal antecedents naturally reduces the remaining variance to be explained by racial change.

The Guest-Zuiches article was based entirely on census tract data However, urban segregation patterns do not have to follow the specific boundaries drawn by the Census Bureau. A partly black census tract can be rigidly segregated by block, though the tract data will mask this. Any microanalysis of neighborhood segregation ought to examine units small enough to reveal the patterns. The Guest-Zuiches article is still valuable, providing that authors and readers bear in mind these limitations.

Further, the authors committed *the fallacy of misplaced sampling inference.* They drew a sample of largely stable, segregated census tracts

and made inferences to a population of unstable, invaded census tracts. Examination of only the 10 tracts which they label as "invaded" leads to very different conclusions. The mean mobility rate of these tracts was 1.186 standard deviations above the mean mobility rate of the whole sample. After the other independent variables were allowed to have their full effect, these tracts were still 0.412 standard deviations above the predicted mean mobility rate. If the "invasion" hypothesis had a fair chance, it would have fared still better. After presenting their statistical analysis, Guest and Zuiches proceed to inspect the 10 changing tracts. They failed to realize the significance of the fact that the two racially changing tracts with the largest number of blacks had the highest mobility rates: 2.619 and 2.495 standard deviations above the mean. Even after allowing the other variables to have their maximum effect, these tracts had mobility rates 1.536 and 1.804 standard deviations above what was predicted. For these tracts, the other variables left very large unexplained residuals. Clearly, a larger sample of changing tracts is needed to make more general inferences.

The correlation between the dummy variable "racial change" (based on coding the 10 "invaded" tracts as 1 and the other tracts as 0) and the migration rate was computed as only .27. This confirms the conclusion that black penetration explains little of the variance in migration from 189 central-city census tracts in Cleveland. However, that should not lead anyone to conclude that black penetration has little effect on white migration from changing neighborhoods. In fact, penetration explained more of the migration from changing tracts than any other variable. The only reason the correlation between racial change and migration was relatively small *over the whole sample* was that there were very few changing tracts.

From these data, we can hardly conclude that racial integration is at hand or that racial segregation occurs by coincidence. On the contrary, it appears that segregation is so rigid that ecological competition between the races does not determine overall migration rates in the inner city. It also appears that whites and blacks can get fairly close before the former are motivated to flee. Guest and Zuiches do teach us something important, namely, that *anticipatory* white flight does not occur in Cleveland very much[1] and that ecological competition between the races does not greatly affect overall migration rates in the inner city. But black movement into white areas, once it has occurred in a specific neighbor-

[1] That is, anticipatory white flight does not take place in adjacent census tracts. However, within a given census tract, anticipatory white flight might be very important in blocks adjacent to "invaded" blocks.

hood, does lead to white flight.[2] If this were not so, how could cities become and remain so segregated?

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REPLY TO FELSON

The utility of extended debate over the emphasis on a minor correlation or an admittedly problematic inference seems questionable. On the other hand, if sociology is to benefit by cumulative empirical research of its substantive problems, this continued dialogue is justified.

In our article (Guest and Zuiches 1971), we argued that Molotch (1969) failed to show that white flight did not occur in South Shore, a racially changing community in Chicago. Given white racial fears, it is possible for whites to move quickly out of a neighborhood, more quickly than would be predicted by a knowledge of the demographic and housing characteristics of the area. Rapid turnover could be encouraged by any variety of incidents, such as panic selling of a few homeowners or the organized actions of realtors.

Our attempt to test the hypothesis of no difference between neighborhoods beyond the conditions of housing and nonracial social characteristics in another metropolitan context provided an extension of Molotch's work from a case study to a multivariate analysis of numerous subareas in a large city, albeit not defined by participant observation; and, in addition, a replication, in a broad interpretation of the word, that could be repeated in other cities; and finally, a generalizability beyond South Shore, Chicago. We showed, moreover, that much of the residential turnover in Cleveland's racially changing neighborhoods could be explained by the demographic and housing character of the areas. Some neighborhoods, however, had unusually high mobility even after controls for the demographic and housing characteristics. We left open the question of why these areas had "excess" turnover.

[2] I leave open the question of *how much* black penetration in a particular neighborhood is necessary to spark a white exodus.

Most of Felson's comments are statistical and methodological, and these will be discussed in the first part of our reply. We shall also discuss our views on the question which seems to be implicit in many of Felson's comments, particularly: what implications do our findings have for the achievement of racial integration in cities?

First, as Felson points out, it is true that we tested the hypothesis that all mobility in racially changing tracts could be explained by nonracial features of the neighborhood. This test of Molotch's hypothesis meant that we determined the mobility rates of changing tracts based on their nonracial characteristics and then compared them with the actual rates to determine whether excessive housing turnover (indicative of white flight) might exist. Although we did not report our results in the paper, we also tested the hypothesis which he suggests—that mobility rates could be explained by both nonracial and racial characteristics (dummy variables for stages of racial change). We determined whether the stages of racial change had any statistical relationship to mobility after we controlled for the nonracial variables. They did, but the conclusions were roughly the same regardless of our manipulation of the data. Felson criticizes us for being unfair to Molotch, but we were trying to be fair by testing Molotch's hypothesis. Moreover, it is not a question of fairness, but of replication and extension of a significant research endeavor by Molotch.

Second, we do not believe that several serious errors of causal specification were made. Contrary to Felson, we made no claim that high population density and a large amount of industrial land keep people in a neighborhood. The data in our paper and ecological theory would suggest otherwise. We also find arcane the meaning of Felson's contention that high mobility rates cause a decline in the status of an area. We prefer our interpretation that a neighborhood with declining social status should generally become less attractive to its residents.

Furthermore, on the matter of causal specification, Felson seems to be suggesting a "correct" causal model which involves the effects of our "control" variables on the mobility rate and, in turn, the effect of the mobility rate on racial succession (or high mobility causes racial succession). This is a possible model. And, in fact, we would argue that racial change can occur because of high white outmovement in an area. If this is Felson's model, then racial succession becomes the dependent variable, and the last sentence in his paragraph on causal models seems to be completely confused to us. Our own modest empirical work involved the test of another possible model (Molotch's)—that most white outmovement could be explained by nonracial factors.

Third, we agree strongly with Felson that researchers ought to bear in mind the limitations of their ecological units of analysis. In fact, we criticized Molotch for that very point. During our extensive correspon-

dence, Molotch suggested that the next study in this area should be based not on housing units (or areal units) but on individual residential histories, and we would pass this suggestion on to Felson. Straits (1968) analyzed residential succession in this fashion, with some interesting Chicago-specific results.

Fourth, Felson points to a discrepancy between the sample of tracts and the subset that were under consideration as "invasion" tracts. In the first place, only 89 of the 189 tracts were stable, white tracts, and we provided detailed description of our criteria for including and categorizing tracts; second, by including tracts with a full range of racial compositions, we have as a standard the average mobility for the entire community, irrespective of racial composition or housing characteristics. It is true that the racially changing tracts had high mobility rates, even after controls for nonracial variables, in comparison with the dispersion of mobility rates (standard deviation) for all tracts. But emphasis on this point is deceptive because there was little dispersion of mobility rates in the first place. The mean mobility rate was 54.3%, with a standard deviation of only 9.7. In absolute percentage terms, the predicted mobility rates of racially changing tracts were not very different, on the average, from the actual rates for those tracts, that is, only four percentage points lower

Fifth, it is true that the two tracts with the greatest excess in mobility also had the highest percentage nonwhite population in 1960. Contrary to Felson, we do mention this in the last paragraph on page 465. There is, however, little systematic relationship with the percentage nonwhite in 1960 and the differences between actual and predicted turnover.

Sixth, we did not conclude that black movement into a tract had little effect on mobility in relationship to any other variable. We made no statement on that point. Our concern was primarily with the effect of racial versus nonracial variables in explaining mobility. Furthermore, we made nothing in the paper of the low correlation (r) between the dummy variable for "invasion" tract and the mobility rate. Felson's interpretation of that correlation is correct; we never argued otherwise.

Seventh, Felson points out that "we should not conclude that racial integration is at hand." We did not conclude that. However, our results do have some implications for understanding the possibilities of achieving racial integration.

Implications

Molotch's paper and ours suggest that greater racial integration of cities is a possibility given the existence of structural conditions in racially changing neighborhoods. Both papers also made thinly veiled references in the last paragraphs to other factors which might affect the possibility

of "white flight" in neighborhoods and the speed of racial change. We would briefly present a framework for understanding how more neighborhood integration might occur in cities, based on the general goal of stabilizing racially changing neighborhoods. Using the racially changing neighborhood as a partial focus of racial integration schemes is not a farfetched idea, since the process of ghetto expansion, particularly in the North, occurs primarily through movement into older white-occupied areas. The outline of our scheme is presented in table 1.

Any goal of neighborhood racial integration must deal with four components of population movement: the inflow and outflow of blacks and the inflow and outflow of whites into a neighborhood. To maintain at any point in time a racially integrated neighborhood, we would want to maximize the inflow of whites and minimize their outflow. High inflow of whites into racially changing neighborhoods is particularly important, since it appears that much of racial change is due to the failure of whites to move in rather than any panic outmigration. This is suggested both by Molotch's article and by our article, and is also implicit in work by Wolf and Lebeaux (1969) and Mayer (1960) on Detroit neighborhoods.

As a general rule, we would want to minimize but not eliminate the inflow of blacks into specific transition neighborhoods. Goals for the fourth population component, the outflow of blacks, are more difficult to determine. If the outflow of blacks became high, the neighborhood would become all white again. On the other hand, if the outflow became very low, a minimal inflow of blacks would still lead eventually to an all-black area.

There are, in turn, general types of policies which could be used to achieve these types of population movement in transition neighborhoods. Some are readily applicable in today's society. Some of these policies are also discussed by Wolf and Lebeaux (1969, pp. 501–21).

In the area of housing policies, it appears that much of the black inflow could be stemmed by the development of alternative housing opportunities. As the Taeubers (1965, pp. 4–5) point out, housing in transition areas tends to be better in quality and space than housing in the established ghetto. Black inmigration into transition areas is particularly selective of higher-status families and families of child-bearing age (Edwards 1972; Guest 1970), and thus alternative housing policies should be directed toward these groups. This type of goal would seem to be relatively feasible in our society. Hopefully, these alternatives, assisted by open-housing policies in new suburban developments to eliminate dependence on the "filter down" effect in the real estate market and increased access to mortgage financing, would be developed so they would also lead to residential integration in other neighborhoods.

It also appears that housing in transition neighborhoods is less attrac-

TABLE 1

POSSIBLE POLICIES TO ACHIEVE NEIGHBORHOOD RACIAL STABILIZATION

Population Flow	Goal	Housing Policy	Institution Policy	Demographic Policy
1. White inmigration	Maximize	Neighborhood rehabilitation, rent supplements	Improve stores, schools, recreation areas	None feasible
2. White outmigration	Minimize	As above	As above	None feasible
3. Black inmigration	Minimize	Create more housing opportunities outside changing neighborhoods, particularly for certain types of families	Possibly increase facilities outside transition neighborhood	Disperse black population to more cities
4. Black outmigration	Moderate amounts	As above	As above	As above

tive to whites than alternative housing opportunities. As a result, the outflow of whites could be stemmed by decreasing the amount of new, quality housing available to the white population outside the changing area. However, this goal is clearly not feasible in a political sense. Nevertheless, it would be possible to *increase* the attractiveness of white housing in changing neighborhoods relative to other white housing through special policies such as rehabilitation, liberal mortgage terms, and special rent supplements to families willing to live in integrated neighborhoods. Some of these factors are discussed by Rapkin and Grigsby (1960, pp. 114–21).

A second policy area would involve community institutions. Some evidence (Mayer 1960) suggests that whites leave changing areas due to the lack of good stores, shopping centers, and schools. A prime goal would thus be the improvement of these facilities in racially changing areas, benefiting all residents in the area.

A third policy area, the demographic, would focus on the flows of blacks and whites into the city itself. As the Taeubers (1965, p. 4) point out, neighborhoods must change racial character if the black population of a city continues to expand, since almost all housing possibilities are located in changing neighborhoods. In 1970, four out of every 10 Negroes in the United States were living in the 30 cities with the largest Negro population (U.S. Bureau of the Census 1971, p. 17). This percentage of the total Negro population of the United States residing in the 30 selected cities has shown a steady increase from 30% in 1960. Thus, a feasible policy for residential integration would attempt to shift the flow of the black population to other cities so the pressure on housing supply does not grow. This policy, of course, could be implemented by such grandiose schemes as "new towns" or by more mundane alternatives such as creating employment opportunities in already existing (primarily white) cities.

Of course, these proposals apply primarily to the stabilization of racially changing neighborhoods. The stabilization of these neighborhoods would make only a small dent in the pervasive pattern of racial residential segregation in American cities. And the achievement of real residential integration, on racial grounds alone or on both racial and socioeconomic grounds, will require much broader policies than we have sketched here. Racial residential segregation is in the final analysis only one aspect of broad types of American racial practices which must be ended. Anthony Downs (1968) has developed, with a much wider scope than simply transition neighborhoods, alternative programs encompassing both dispersal and enrichment policies for the American ghetto.

Since publication of our original work on Cleveland, we have been investigating the speed of neighborhood racial change and rates of white

outmovement in various changing neighborhoods. There appear to be some striking variations in these patterns over cities. Perhaps eventually we shall be able to report in more detail the nature of these patterns and the reasons for them.

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WHY NEIGHBORHOODS CHANGE: A REPLY TO WHOM IT MAY CONCERN

I tried to demonstrate (Molotch 1969) that black immigration, once it does occur, does *not* necessarily lead to white flight—even when it does lead to relatively rapid racial transition. Based on a single case study, my findings are necessarily bounded by time, place, and other circumstances. I took my results to indicate that, unlike the 1940s and 1950s, the period upon which the bulk of our racial-change literature is based, new interethnic dynamics were at work in the middle and late 1960s which inhibited certain manifestations of white racism. The Guest and

Zuiches study is based on 1950s data. That their results seemed to confirm my original findings came to me as a (happy) surprise.

Felson's arguments lead me to think that indeed, the South Shore pattern represents a new trend in racial-change processes. But Felson's final question leads me to think that he considers this new pattern a logical impossibility. For Felson, whites must continue to flee changing communities, or else "how could cities become and remain so segregated?" I want to attempt an answer to this interesting question.

Segregation persists not because whites flee but because blacks are discriminated against. Discrimination against blacks means that there are two separate, parallel housing markets in American cities, one white, one black, each with its own supply-demand relationship. The two markets are not symmetrical in that while whites have access to the entire housing stock of a metropolitan area, blacks are more severely curtailed in their purchasing/renting opportunities.[1] Thus, the supply-demand ratio is higher for whites than for blacks; that is, prices (rents and purchases) are more costly to blacks than to whites by virtue of the fact that a large portion of the available housing stock is arbitrarily removed from the Negro market. A changing area is, by this perspective, an arena where the Negro and white housing markets merge. In this kind of setting, any units which may fall vacant, and which are available to both races, will fetch higher prices from blacks than from whites by dint of market circumstances. Thus, such units will become disproportionately occupied by blacks, and hence the community will, over time, undergo a complete transition from white to black. White flight will accelerate this process by increasing the number of vacancies and hence the number of units subject to this dynamic, but such flight is not necessary for the basic process to occur.

Under present conditions, a situation of stable racial integration can come about only when arbitrary racial quotas are fixed and enforced through administrative intervention (as is common in urban-renewal areas) or where there is a *peculiar* attractiveness of a community to whites but not to blacks. In such a setting (e.g., Hyde Park adjacent to the University of Chicago), integration is made possible because, in effect, everybody pays Negro prices.

The reason this model may not seem obvious is because it rests upon the sometimes forgotten fact that urban communities are highly mobile

[1] This disparity between the two housing markets is documented through a survey of Chicago-area real estate personnel, as well as other methods described elsewhere (Molotch 1972). The model of racial change described here is also consistent with the arguments and data of Taeuber and Taeuber (1965, esp. p. 25) and, as argued in Molotch (1972), with the great bulk of relevant evidence on housing prices and race (e.g., Laurenti 1960; Muth 1969) and consumer racial behavior (e.g., Grier and Grier 1960).

places, the protestations of a relatively small number of community "old-timers" notwithstanding. Approximately one out of every five American households change location every year. If such moving behavior were evenly distributed among the population, a complete transition of a community could occur over a mere five-year period without anybody fleeing from anybody. It takes somewhat longer because such moving behavior is not evenly distributed and because a declining proportion of the replacement purchasing and renting is by whites moving into a changing area (cf. Rapkin and Grigsby 1960). I suspect that some "racial moving" does occur, but less and less. This is the racial-change pattern which I think I witnessed in South Shore and which I think may be the general emerging pattern of the 1970s. To get back to Felson's question, segregation will persist but white flight may not be the cause. As I argue elsewhere (Molotch 1972, chap. 10), the policy implication is that stable racial integration is essentially contingent upon elimination of discrimination and all other forces which inhibit blacks from gaining access to good housing. I doubt there are any shortcuts.

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REMARKS ON RUSHING'S PAPER ON HOSPITAL COMMITMENT

Publication of "Individual Resources, Societal Reaction, and Hospital Commitments" by William Rushing in the November 1971 issue of the *AJS* places the author and me in an embarrassing situation. The article is a replication and extension (although it is presented as original analysis) of my previously published article, "Who Shall Be Excluded: The Influence of Personal Attributes in Community Reaction to the Mentally Ill," which appeared in the July 1970 issue of *Social Psychiatry*.

I feel the need to point out what I consider to be the major points of similarity between Professor Rushing's article and my own:

a) Rushing's major hypothesis, which he refers to as his "resource-deprivation hypothesis," is stated as follows: "Individuals with limited social and economic resources have limited power and are more apt to be involuntarily confined than individuals whose resources and power are less limited." This hypothesis is further specified in the next paragraph: "Social and economic resources are assumed to be inversely related to an individual's socioeconomic status" and, "As for marital status, resources are assumed to be highest for the married and lowest for the single, with the disrupted-estranged (divorced, separated and widowed) in between" (Rushing, p. 513).

Compare with "Hypothesis One and Two," from my earlier article (Linsky 1970*b*): (1) "Communities Have a Greater Propensity to Exclude for Deviance Lower Class Persons and Members of Low Status Racial-Ethnic Groups," and (2) "Communities Have Greater Propensity to Exclude for Deviance Persons Who Lack Close Social Ties in the Community." The latter hypothesis is explained further in the paragraph following: "Isolated individuals and those without families would be expected to be institutionalized more readily than others because they lack social ties and sheltering primary type relationships to fall back on in difficult situations" (pp. 167, 169).

b) Community rejection of deviants, the dependent variable throughout the study (in both articles) is operationalized by Rushing by computing the ratio of involuntary to voluntary admissions (pp. 512, 519). This index forms the major basis of his analysis. Although it is identical with the index I used in "Who Shall Be Excluded" (p. 167), it is not acknowledged. I developed this measure named the "Exclusion Index" in a much earlier report (Linsky 1966), and have used it recently in another study of societal reaction to mental illness (Linsky 1970*a*, p. 307).

c) Rushing has added some measures of independent variables not included in my earlier paper. He uses occupational status, including farm and nonfarm categories, and combines them into five socioeconomic strata based upon occupational prestige as indicators of powerlessness and marginality (pp. 518, 519), whereas my earlier article used educational level and racial-ethnic status to get at the same thing (Linsky 1970*b*, pp. 168–69). Rushing uses the categories "married, disrupted-estranged, and single," (p. 513) compared with "married and non-married" and "family living situation vs. non-family living situations" in my paper (1970*b*, p. 169). He goes on to consider the influence of marital and SES status taken jointly as well as separately. These additional measures are quite useful but they do not change the basic framework of the study.

d) Rushing reports essentially the same findings as that reported in the

earlier article. This is reassuring but hardly surprising since he tests essentially the same hypotheses with the same design on *part of the same patient population used in the earlier study.* Rushing's data is based on male first admissions to the three Washington state mental hospitals, 1956 through 1965 (p. 518), whereas the patient population for my study was based on all first admissions to the same three Washington state hospitals for the period 1957–64 (1970*b*, p. 167).

Rushing does reference my paper (pp. 511–12), but the reference in no way communicates that his study is a repeat or at the outside, a refinement of my earlier study. It is presented instead as an original analysis.

I believe Rushing should have indicated that his article represents in part a repeat of my analysis based on the male portion of the patient population and in part an extension by the introduction of additional independent variables beside those included in my analysis.

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RUSHING REPLIES

In my recent paper (1971*b*) I failed to acknowledge that in an earlier paper Arnold Linsky (1970) investigated some of the same bivariate relationships that I investigated. In particular, Linsky investigated the relationships of marital status and education to the involuntary/voluntary ratio for patients in the state mental hospitals in Washington. I also investigated the relationship for marital status, and my measure of occupational status, based on Duncan's transformed NORC values, is probably correlated with education. Although I did note that Linsky had dealt with the same dependent variable (the involuntary/voluntary ratio), I failed to acknowledge the other similarities. This was due to an oversight for which I am embarrassed. (Professor Linsky need not be embarrassed by this, however; it is *my* error.)

At the same time, Linsky seriously overstates the case. There are important differences between our papers which he does not acknowledge. They should be reported just to set the record straight.

Theoretical objectives and interpretation.—In my paper I investigated the resource deprivation hypothesis which I derived from the hypothesis of cumulative social disadvantage (Lipset and Bendix 1959, p. 196); this hypothesis has been central in my work for several years (1970 and 1971*a*). I conceptualize class and marital status as *one* variable, individual resources, and argue that there are good theoretical and empirical grounds for believing that this variable is related to hospital commitment, as the hypothesis of cumulative social disadvantage implies. Over one-third of my paper (pp. 513–18) was devoted to these issues. In addition, my paper dealt with rates as well as ratios, and I argued that the influence of community isolation is different in the two cases. None of these issues is addressed in Linsky's paper. The intellectual sources of the two papers are clearly different; nothing Linsky said in his paper had any influence on anything I said in mine.

Analysis of the data.—As in a previous publication on hospitalization rates (1969), my concern in examining the bivariate relationships between resource deprivation and involuntary/voluntary ratios was to show the presence of *two* patterns in the relationship; the ratio continuously decreases as resources decrease, but is disproportionately high when resources are extremely low. An even greater concern was to investigate the resource deprivation hypothesis in terms of the interaction effects between the two individual resource variables (class and marital status) and between individual resources and community isolation visibility. None of these issues is dealt with in Linsky's paper. They are central in mine. Almost one-fourth of my paper deals with interaction effects alone (pp. 522–24).

The data.—Similarities of data in our papers are less than Linsky claims. Linsky's data for education are derived from official statistics and reported for only 36% of the cases, whereas I reported the occupation for 84% of the cases, based on the examination of patients' original hospital records; he includes males and females in the analysis; and he includes an unknown number of persons over 65 and under 21 (my findings are for persons 21–65 only). Consequently, it is not possible to know how much Linsky's results reflect errors in official statistics, the failure to distinguish between males and females (his *own* analysis reveals sexual differences in the ratio [Linsky 1970, table 3]), and the inclusion of older persons who probably contribute disproportionately to the hospitalization rate (persons over 65 probably have less formal education and higher rates of widowhood than persons 65 and less). Linsky does not recognize the importance of my

distinction between the single and the disrupted-estranged; if the hospitalization rate were higher for the latter, the resource deprivation hypothesis as I formulated it would be called into question.

So despite certain surface similarities in our two papers, there are major differences which most readers will recognize. This does not justify my failure to make appropriate reference to the similarities, of course. But to consider my paper a mere replication of Linsky's is an exaggeration, to say the least.

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ERRATUM TO THE WERTS-LINN COMMENTS ON BOYLE'S "PATH ANALYSIS AND ORDINAL DATA"[1]

Werts and Linn (1971) pointed out that Boyle (1970) had implicitly assumed that the causative variables were measured without error. Further study of literature relating to this problem (e.g., Cochran 1968; Evans 1970; Anderson 1959) indicates that the Werts-Linn procedure for dealing with categorical errors of measurement is incorrect. The purpose of this note is to set the record straight.

As a basis for generalization to polychotomous variables, first consider the case of three independent fallible dichotomous measures X_j ($j = 1, 2, 3$) of an underlying true dichotomy (T). The observed categories will be labeled $k = 1, 2$ and the true categories $l = 1, 2$. The relationship between X_j and T can be expressed as a function of the conditional probabilities $P(X_j = k|T = l) = \theta_{jkl}$ for each combination of k and l:

$$P(X_j = 1|T = 1) = \theta_{j11},\ P(X_j = 1|T = 2) = \theta_{j12},$$

$$P(X_j = 2|T = 1) = \theta_{j21}, \quad \text{and} \quad P(X_j = 2|T = 2) = \theta_{j22}; \quad \text{where}$$

θ_{j21} is commonly labeled the proportion of false negatives and θ_{j12} the proportion of false positives. The sum of the conditional probabilities for a fixed value of l is unity (i.e., $\theta_{j11} + \theta_{j21} = 1$ and $\theta_{j12} + \theta_{j22} = 1$). Define $P_{T_l} = P(T = l)$ and $P_{j_k} = P(X_j = k)$ where

$$\sum^{l} P_{T_l} = 1$$

and

$$\sum^{k} P_{j_k} = 1.$$

The model parameters to be estimated are the conditional probabilities for each observed measure and the true proportions in each category. Since each object is categorized by each different measure, the proportion of objects for each combination of observed categories can be computed. Define $P_{jk,j'k',j''k''} = P(X_j = k, X_{j'} = k', X_{j''} = k'')$ where $j \neq j' \neq j''$. In the three-measure case the observed data consist of eight joint probabilities $P_{11,21,31}$, $P_{11,21,32}$, $P_{11,22,31}$, $P_{11,22,32}$, $P_{12,21,31}$, $P_{12,21,32}$, $P_{12,22,31}$, and $P_{12,22,32}$ The next step is to relate these observed probabilities to the model parameters. Starting with $P_{11,21,31}$ we obtain:

$E(P_{11,21,31}) = P(X_1 = 1,\ X_2 = 1,\ X_3 = 1,\ T = 1) + P(X_1 = 1,\ X_2 = 1, X_3 = 1, T = 2)$. Expressed in terms of conditional probabilities, the proportions are $P(X_1 = 1,\ X_2 = 1,\ X_3 = 1,\ T = 1) = P(X_1 = 1|X_2 =$

[1] The research reported herein was performed pursuant to grant OEG-2-700033(509) with the U.S. Department of Health, Education, and Welfare and the Office of Education.

$1,\ X_3 = 1,\ T = 1)P(X_2 = 1|X_3 = 1,\ T = 1)P(X_3 = 1|T = 1)P(T = 1)$, and $P(X_1 = 1,\ X_2 = 1,\ X_3 = 1,\ T = 2) = P(X_1 = 1|X_2 = 1,\ X_3 = 1,\ T = 2)P(X_2 = 1|X_3 = 1,\ T = 2)P(X_3 = 1|T = 2)P(T = 2)$.

The assumption that the measures are independent implies that

$$P(X_1 = 1|X_2 = 1, X_3 = 1, T = 1) = P(X_1 = 1|T = 1) = \theta_{111},$$

$$P(X_2 = 1|X_3 = 1, T = 1) = P(X_2 = 1|T = 1) = \theta_{211},$$

$$P(X_1 = 1|X_2 = 1, X_3 = 1, T = 2) = P(X_1 = 1|T = 2) = \theta_{112},$$

and

$$P(X_2 = 1|X_3 = 1, T = 2) = P(X_2 = 1|T = 2) = \theta_{212}.$$

Thus, by substitution:

$$E(P_{11,21,31}) = \theta_{111}\,\theta_{211}\,\theta_{311}\,P_{T_1} + \theta_{112}\,\theta_{212}\,\theta_{312}\,P_{T_2}. \tag{1}$$

While this process could be repeated for each of the observed joint probabilities, for identification purposes it is better to replace these by the following set:

$$P_{11,21} = P_{11,21,31} + P_{11,21,32},$$

$$P_{11,31} = P_{11,21,31} + P_{11,22,31},$$

$$P_{21,31} = P_{11,21,31} + P_{12,21,31},$$

$$P_{11} = P_{11,21,31} + P_{11,21,32} + P_{11,22,31} + P_{11,22,32},$$

$$P_{21} = P_{11,21,31} + P_{11,21,32} + P_{12,21,31} + P_{12,21,32},$$

and

$$P_{31} = P_{11,21,31} + P_{11,22,31} + P_{12,21,31} + P_{12,22,31}.$$

Following the procedure used for $P_{11,21,31}$ it may be shown that:

$$E(P_{11,21}) = \theta_{111}\,\theta_{211}\,P_{T_1} + \theta_{112}\,\theta_{212}\,P_{T_2}, \tag{2}$$

$$E(P_{11,31}) = \theta_{111}\,\theta_{311}\,P_{T_1} + \theta_{112}\,\theta_{312}\,P_{T_2}, \tag{3}$$

$$E(P_{21,31}) = \theta_{211}\,\theta_{311}\,P_{T_1} + \theta_{212}\,\theta_{312}\,P_{T_2}, \tag{4}$$

$$E(P_{11}) = \theta_{111}\,P_{T_1} + \theta_{112}\,P_{T_2}, \tag{5}$$

$$E(P_{21}) = \theta_{211}\,P_{T_1} + \theta_{212}\,P_{T_2}, \tag{6}$$

and

$$E(P_{31}) = \theta_{311}\,P_{T_1} + \theta_{312}\,P_{T_2}. \tag{7}$$

Note that even though we started with eight joint probabilities, we have only seven equations because of the condition that all the observed prob-

abilities sum to unity. If the model parameters are identifiable, then it should be possible to solve these equations for each parameter in terms of the expected probabilities. For this purpose it is convenient to define: $C_{jk,j'k'} = E(P_{jk,j'k'}) - E(P_{jk})E(P_{j'k'})$, $C_{jk,j'k',j''k''} = E(P_{jk,j'k',j''k''}) - [E(P_{jk})]\ C_{j'k',j''k''} - [E(P_{j'k'})]\ C_{jk,j''k''} - [E(P_{j''k''})]\ C_{jk,j'k'} - E(P_{jk})\ E(P_{j'k'})E(P_{j''k''})$, and $Q_{T_l} = 1 - P_{T_l}$. For the dichotomous case $Q_{T_1} = P_{T_2}$. Solving equations (1) through (7) for P_{T_1} we obtain:

$$\frac{Q_{T_1}^2 - P_{T_1}^2}{\sqrt{P_{T_1}Q_{T_1}}} = \frac{C_{11,21,31}}{\sqrt{C_{11,21}\,C_{11,31}\,C_{21,31}}}. \tag{8}$$

Equation (8) shows that P_{T_1} and $P_{T_2} = 1 - P_{T_1}$ are identified. Further analysis yields:

$$\theta_{112} = E(P_{11}) - \sqrt{\frac{C_{11,21}\,C_{11,31}}{C_{21,31}}\left(\frac{P_{T_1}}{Q_{T_1}}\right)}, \tag{9}$$

$$\theta_{212} = E(P_{11}) - \sqrt{\frac{C_{11,21}\,C_{21,31}}{C_{11,31}}\left(\frac{P_{T_1}}{Q_{T_1}}\right)}, \tag{10}$$

and

$$\theta_{312} = E(P_{11}) - \sqrt{\frac{C_{11,31}\,C_{21,31}}{C_{11,21}}\left(\frac{P_{T_1}}{Q_{T_1}}\right)}. \tag{11}$$

Since P_{T_1} and P_{T_2} are identified, equations (9), (10), and (11) show that θ_{112}, θ_{212}, θ_{312}, and therefore $\theta_{122} = 1 - \theta_{112}$, $\theta_{222}, = 1 - \theta_{212}$, and $\theta_{322} = 1 - \theta_{312}$ are identifiable. Given P_{T_1}, P_{T_2}, θ_{112}, θ_{212}, and θ_{312} identified, equations (5), (6), and (7) show that θ_{111}, θ_{211}, and θ_{311} and therefore $\theta_{121} = 1 - \theta_{111}$, $\theta_{221}, = 1 - \theta_{211}$, and $\theta_{321} = 1 - \theta_{311}$ are identifiable. Since the model consists of seven equations in seven unknowns (i.e., just identified), parameter estimates can be obtained which will exactly reproduce the observed probabilities, that is, the observed joint probabilities would equal the expected joint probabilities estimated from the parameter estimates. The above analysis shows that the true proportions may be identified given three independent dichotomous measures, a point which Werts and Linn (1971) failed to discover. The right-side numerator of equation (8) is the expected value of the triple covariance between X_1, X_2, and X_3, which is the crucial piece of information neglected in the Werts-Linn path approach. Furthermore, path analysis usually ignores variable means, which would result in neglect of equations (9), (10), and (11) which involve means (P_{jk}).

Next consider the trichotomous case in which $k = 1,2,3$, $l = 1,2,3$ and $j = 1,2,3$ given the assumption of independent measures. The relationship between the jth observed trichotomy and the true trichotomy involves nine

conditional probabilities: θ^*_{j11}, θ^*_{j12}, θ^*_{j21} and θ^*_{j22} as defined previously plus $P(X_j = 1|T = 3) = \theta^*_{j13}$, $P(X_j = 2|T = 3) = \theta^*_{j23}$, $P(X_j = 3|T = 1) = \theta^*_{j31}$, $P(X_j = 3|T = 2) = \theta^*_{j32}$, and $P(X_j = 3|T = 3) = \theta^*_{j33}$. By definition: $\theta^*_{j11} + \theta^*_{j21} + \theta^*_{j31} = \theta^*_{j12} + \theta^*_{j22} + \theta^*_{j32} = \theta^*_{j13} + \theta^*_{j23} + \theta^*_{j33} = 1$. Let $K =$ total number of categories and $J =$ total number of independent measures. The observed data consists of the $K^J = 27$ joint triple probabilities $P_{1k,\,2k',\,3k''}$, one of which may be expressed as a function of the other 26. There are $JK^2 = 27$ θ^*_{jkl}, JK of which can be stated as a function of the others because for a fixed l the θ^*_{jkl} sum to unity and $K = 3$ P_{T_l}, one of which it can be stated as 1 minus the sum of the others. Therefore there are a total of $JK(K-1) + (K-1) = 20$ independent parameters to be estimated from the $K^J - 1 = 26$ independent observed joint probabilities (i.e., the model has six overidentifying restrictions). This does not necessarily mean that all parameters are identified, and in principle the expected value of each $P^*_{jk,j'k',j''k''}$ should be derived as done previously and the equations solved for each parameter. Rather than attempt this directly, it can be seen that if category 3 were collapsed into category 2, then the analysis would be identical with that shown for dichotomous variables. The relationships would be (* refers to probabilities prior to collapsing categories):

$$P_{T_1} = P^*_{T_1}, \tag{12a}$$

$$\theta_{j11} = \theta^*_{j11}, \tag{12b}$$

and

$$\theta_{j12}(1 - P_{T_1}) = \theta^*_{j12}\, P^*_{T_2} + \theta^*_{j13}\, P^*_{T_3}. \tag{12c}$$

From our previous analysis we know that the parameters in the right side of equations (12a)–(12c) can be identified from

$$P_{11,21,31} = P^*_{11,21,31},$$

$$P_{11,21,32} = P^*_{11,21,32} + P^*_{11,21,33},$$

$$P_{11,22,31} = P^*_{11,22,31} + P^*_{11,23,31},$$

$$P_{11,22,32} = P^*_{11,22,32} + P^*_{11,23,32} + P^*_{11,22,33} + P^*_{11,23,33},$$

$$P_{12,21,31} = P^*_{12,21,31} + P^*_{13,21,31},$$

$$P_{12,21,32} = P^*_{12,21,32} + P^*_{12,21,33} + P^*_{13,21,32} + P^*_{13,21,33},$$

$$P_{12,22,31} = P^*_{12,22,31} + P^*_{12,23,31} + P^*_{13,22,31} + P^*_{13,23,31},$$

and

$$P_{12,22,32} = P^*_{12,22,32} + P^*_{12,22,33} + P^*_{12,23,32} + P^*_{12,23,33} + P^*_{13,23,32} + P^*_{13,22,33} + P^*_{13,23,32} + P^*_{13,23,33}.$$

These eight $P_{jk,\,j'k',\,j''k''}$ could be entered into the analysis shown for dichotomies and the corresponding parameters in (12a), N(12c) identified. In a similar fashion if we collapse category 1 into 3 then:

$$P_{T_2} P^*_{T_2}, \tag{12d}$$

$$\theta_{j22} = \theta^*_{j22}, \tag{12e}$$

and

$$\theta_{j23}\,(1 - P_{T_2}) = \theta^*_{j21}\,P^*_{T_1} + \theta^*_{j23}\,P^*_{T_3}. \tag{12f}$$

The right-hand parameters in (12d) N(12f) would be identified from:

$$P_{12,22,32} = P^*_{12,22,32},$$

$$P_{12,22,33} = P^*_{12,22,31,} + P^*_{12,22,33},$$

$$P_{12,23,32} = P^*_{12,23,32} + P_{12,21,32},$$

$$P_{12,23,33} = P^*_{12,23,33} + P^*_{12,21,33} + P^*_{12,23,31} + P^*_{12,21,31},$$

$$P_{13,22,32} = P^*_{13,22,32} + P^*_{11,22,32},$$

$$P_{13,22,33} = P^*_{13,22,33} + P^*_{13,22,31} + P^*_{11,22,33} + P^*_{11,22,31},$$

$$P_{13,23,32} = P^*_{13,23,32} + P^*_{13,21,32} + P^*_{11,23,32} + P^*_{11,21,32},$$

and

$$P_{13,23,33} = P^*_{13,23,33} + P^*_{13,23,31} + P^*_{13,21,33} + P^*_{13,21,31} + P^*_{11,23,33} + P^*_{11,23,31} + P^*_{11,21,33} + P^*_{11,21,31}.$$

These eight $P_{jk,j'k',j''k''}$ could likewise be entered into the analysis for dichotomies where the two categories are $k = 2,3$ instead of $k = 1,2$ as shown in our original analysis. We can conclude that $P^*_{T_1}$, $P^*_{T_2}$, and $P^*_{T_3}$ are identified from (12a) and (12d) and θ^*_{111}, θ^*_{211}, θ^*_{311}, θ^*_{122}, θ^*_{222}, and θ^*_{322} from equations (12b) and (12e). The remaining 12 parameters in equations (12c) and (12f) have six conditions imposed by equations (12c) and (12f), so we need six more equations for identification. The simplest set, which is independent of information used in the dichotomous analyses is:

$$P_{11,22} = P^*_{11,22,31} + P^*_{11,22,32} + P^*_{11,22,33},$$

$$P^*_{11,32} = P^*_{11,21,32} + P^*_{11,22,32} + P^*_{11,23,32},$$

$$P^*_{12,21} = P^*_{12,21,31} + P^*_{12,21,32} + P^*_{12,21,33},$$

$$P^*_{12,31} = P^*_{12,21,31} + P^*_{12,22,31} + P^*_{12,23,31},$$

$$P^*_{21,32} = P^*_{11,21,32} + P^*_{12,21,32} + P^*_{13,21,32},$$

and

$$P^*_{22,31} = P^*_{11,22,31} + P^*_{12,22,31} + P^*_{13,22,31}.$$

Application of the procedure used to derive equation (1) yields:

$$\left.\begin{aligned}
E(P^*_{11.22}) &= \theta^*_{111}\theta^*_{221}P^*_{T_1} + \theta^*_{112}\theta^*_{222}P^*_{T_2} + \theta^*_{113}\theta^*_{223}P^*_{T_3},\\
E(P^*_{11.32}) &= \theta^*_{111}\theta^*_{321}P^*_{T_1} + \theta^*_{112}\theta^*_{322}P^*_{T_2} + \theta^*_{113}\theta^*_{323}P^*_{T_3},\\
E(P^*_{12.21}) &= \theta^*_{121}\theta^*_{211}P^*_{T_1} + \theta^*_{122}\theta^*_{212}P^*_{T_2} + \theta^*_{123}\theta^*_{213}P^*_{T_3},\\
E(P^*_{12.31}) &= \theta^*_{121}\theta^*_{311}P^*_{T_1} + \theta^*_{122}\theta^*_{312}P^*_{T_2} + \theta^*_{123}\theta^*_{313}P^*_{T_3},\\
E(P^*_{21.32}) &= \theta^*_{211}\theta^*_{321}P^*_{T_1} + \theta^*_{212}\theta^*_{322}P^*_{T_2} + \theta^*_{213}\theta^*_{323}P^*_{T_3},\\
&\text{and}\\
E(P^*_{22.31}) &= \theta^*_{221}\theta^*_{311}P^*_{T_1} + \theta^*_{222}\theta^*_{312}P^*_{T_2} + \theta^*_{223}\theta^*_{313}P^*_{T_3}.
\end{aligned}\right\} \quad (13)$$

Equations (13) in combination with previously identified parameters and equations (11), (12c) and (12f) identify the remaining parameters. Note that six equations have not been used, these representing the six degrees of overidentification. The method which appears appropriate for estimating parameters when the observed variables are independent polychotomous measures is discussed in Anderson (1959, sec. 3.6) and Cochran (1968, sec. 6). In this procedure a χ^2 function involving the observed and estimated expected joint probabilities is minimized as a function of the model parameters. The resulting χ^2 with degrees of freedom equal to the number of overidentifying restrictions is a measure of the fit of the model to the data. Our analysis indicates that given three independent polychotomies ($K = 3$), all model parameters are identifiable. The number of overidentifying restrictions is equal to $(K^J - 1) - (JK + 1)(K - 1)$ where $J =$ the number of independent measures and $K =$ the number of categories.

We may now consider exactly why the Werts-Linn analysis was inappropriate to the problem. For this purpose it is helpful to put the conditional probabilities into matrix form where columns refer to observed categories and rows to true categories:

$$\theta^*_j = \begin{bmatrix} \theta^*_{j11} & \theta^*_{j21} & \theta^*_{j31} \\ \theta^*_{j12} & \theta^*_{j22} & \theta^*_{j32} \\ \theta^*_{j13} & \theta^*_{j23} & \theta^*_{j33} \end{bmatrix}. \quad (14)$$

As noted earlier, each row in θ^*_j sums to unity. If the true categories 1, 2, and 3 actually form an ordered set of classifications such that category 1 is closer to 2 than to 3, then we would expect that classificatory errors would be more likely for neighboring categories (i.e., $\theta^*_{j12} > \theta^*_{j13}$ and $\theta^*_{j32} > \theta^*_{j31}$). In contrast, if the true categories are basically unordered, it would be more reasonable to expect the likelihood of misclassification to be similar for any of the other classes (i.e., $\theta^*_{j12} \cong \theta^*_{j13}$, $\theta^*_{j21} \cong \theta^*_{j23}$,

and $\theta^*_{j31} \cong \theta^*_{j33}$). In other words the probability of misclassification is a function of the underlying scale or true category in the case of ordered categories and is not in the case of unordered categories. Werts and Linn implicitly assumed that the errors for one category were uncorrelated with the underlying true dummy variable for the same and for other categories, which translated into the present framework corresponds to the analysis for an unordered scale, that is, for an ordered scale the errors would be correlated with the true dummy variables for other categories. It can be algebraically shown that the Werts-Linn procedure leads to incorrect formulae for the expected value of the observed joint probabilities when the categories are ordered. Since Boyle (1970) was examining the problem of ordered categories (i.e., scales) the Werts-Linn approach is not relevant to his problem.

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Erratum

In the May 1972 issue of the *Journal*, Ira I. Reiss's book, *The Family System in America*, was listed incorrectly as $19.95. The price is actually $9.95.

Review Symposium

Beyond Freedom and Dignity. By B. F. Skinner. New York: Alfred A. Knopf, Inc., 1971. Pp. 225. $6.95.

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One sometime wonders what all the fuss over B. F. Skinner is about. Take his behavioral psychology itself. Many persons do not realize that they have been talking prose, that is, behavioral psychology, for a very long time, indeed for endless ages. It did not require Skinner to teach people that if they wanted someone to do something, it helped to offer him an inducement, or that a man who had performed an action which was successful in particular circumstances was apt to repeat that action when similar circumstances recurred. This sort of common sense embodies perfectly sound behavioral principles. What modern behavioral psychology has done is state the principles explicitly, free them from unnecessary assumptions, test them rigorously in experiments largely carried out on animals, and begin to establish the crucial quantitative relationships. All this has been a very great achievement, and Skinner has been the leader in it, but it is an achievement in systematizing what men and women have long known intuitively and unsystematically. They could not have helped knowing it. Their own behavior and that of others were what they were most familiar with, and success in social life required that they use behavioral principles in some haphazard fashion, even if they did not formulate them.

I myself am fully persuaded of the truth of the principles of behavioral psychology as so far formulated, though I think there may be principles we have yet to formulate clearly. To this extent, I am a Skinnerian and have long been known as one. But the issue raised by his present book is less the intellectual truth of behavioral psychology than its application in a systematic technology for controlling human behavior. Skinner's central argument is that the views put forward by the ideologists of human freedom and dignity may prevent men from adopting a technology which might be more powerful in changing human behavior and changing it for the better than anything we use now. On this issue, I believe both Skinner and his opponents are much more touchy than the reality warrants, and that the most serious impediment to the adoption of a behavioral technology is one that Skinner fails to recognize. Neither Skinner nor his opponents will make as much practical difference as either party seems to think.

Let me turn first to the famous problem of "the freedom of the will." Its practical import is limited, whatever its philosophical interest may be. As a behaviorist, I must believe that my actions are wholly determined, that the choice I make between alternatives is predestined—but what earthly

difference does it make? My intellectual conviction does not, as I wish it would, relieve me of the necessity of making a decision; and in this thoroughly operational sense I remain a free, autonomous man. Not until a machine is invented that will always and correctly predict my action in advance, even when I know that it is designed to predict my action, shall I lose my de facto freedom and autonomy. I shall then also become superfluous. But even if at some time in the future machines of this sort can be produced, their production is always going to be much more costly than the production of ordinary men like myself by what I shall call conventional methods, and therefore men are never going to loose their loophole to freedom and autonomy. No one need fear that behavioral psychology is going to rob him of his free will, so what is all the fuss about?

Freedom is really a moral demand, not an intellectual issue. Though the philosophic freedom of man's will may be preserved by cost considerations—to prove that the behavior of every man is utterly determined will always cost more than the proof will be worth—nevertheless, men will always in fact attempt to determine, that is, to control, the behavior of others, and their attempts will often be successful. The demand that men have made for themselves and for others that they should be free has usually been a demand not that they should be spared attempts to control their behavior, but only that they should be spared certain kinds of attempts—adversive control, as Skinner calls it—control by threats, deprivation, and force. In this demand Skinner himself concurs. Few men have argued, on the other hand, that men should be free from control in the sense of being spared the temptation of receiving positive rewards for their actions, unless the actions themselves were believed to have adversive consequences in the long run, like Adam's eating the apple.

True, some silly things have been said, and some persons have argued that men should be freed from all control, but the arguments soon get into hopeless difficulties. First, for one man to refrain from attempting to control the behavior of another does not mean that the other's behavior is left uncontrolled: it only means that it comes under the control of different variables. Second, and more interesting, it is always possible, indeed it is easy to show, that the very persons who declaim against efforts to control others—"manipulate" is the word often used—do in fact attempt to control others. Accordingly, they are hypocrites, though no doubt unconscious ones, if that is not a contradiction in terms. Remember what Mr. Justice Holmes had to say about the philosophical objections to "treating man as a thing, and the like. . . . If a man lives in society, he is liable to find himself so treated." He might have added "and so to treat others" (O. W. Holmes, *The Common Law,* ed. M. DeW. Howe [Cambridge, Mass.: Harvard University Press, 1963], p. 38).

Take the case of Arthur Koestler, who ought to know better. Skinner quotes him (p. 165) as referring to behaviorism as "a monstrous triviality." It represents, he says, "question-begging on an heroic scale." It has spun psychology into "a modern version of the Dark Ages." Triviality though it is, one could quite easily show that it has been exceedingly important to

Arthur Koestler, for he uses it every day of his life, for instance when he asks anyone else to do anything for him—for pay or not for pay. "Manipulation" always seems to refer to someone else's attempt to control the behavior of others, not one's own.

Skinner argues that the literature of freedom tends, in fact and contrary to its intent, toward perpetuating adverse control. Thus, if a man is free in the sense that his behavior is not determined by external circumstances, then he may personally be held responsible for his behavior, and if he is responsible, then he may fairly be punished if he does ill.

For myself, I would not hold the literature of freedom so much to blame for perpetuating either aversive or other relatively ineffective methods of control. (Incidentally, Skinner never makes clear what his alternative "strong" or "effective" methods would be.) The fact the very men who most decry behavioral psychology as a threat to freedom will use its principles to control others when they are able to offer these others positive rewards, suggests that something else is at stake. It is easy, for instance, to say that the juvenile delinquent should not be punished but rather that the environment that produces him should be changed. That, indeed, is a reasonable conclusion from behavioral psychology. But we may not be able to change the environment, or the cost of doing so may be more than we are willing to pay. We may, on the other hand, be able to punish the delinquent. Our action at least satisfies our emotional need for vicarious revenge, and it may have some little effect for the good on his future behavior and that of others. So we punish him, and we cite his freedom in order to rationalize our action. That is, it may not be the literature of freedom that perpetuates aversive and other weak forms of control but, rather, our aversive and other weak forms of control that perpetuate the literature of freedom. And, as I shall try to show later, the fact that control is carried out according to the principles of behavioral psychology need not exempt it from a crucial kind of weakness.

I feel even more oversold in what Skinner says about dignity than in what he says about freedom. I think both he and those who oppose him exaggerate the dangers each presents to the other. What Skinner means by dignity is the credit men give to others (and take for themselves) for the good deeds they have performed. Skinner argues cogently that men are apt to give others credit for actions that have the following two properties: they reward the credit givers, and the conditions that determine them are not easily recognizable. (Thus, we would not give a man credit for bravery in jumping from a high place if we could see that he had been pushed.) I myself would state in a somewhat different fashion the characteristic that makes an action praiseworthy: the action must be rare, in short supply relative to the demand for it. But I suspect that this amounts to saying the same thing, for the actions whose determinants are readily recognizable are just the ones that are easily produced, that many people can and do perform.

Accordingly, the more clearly one can demonstrate the conditions and laws that determine the actions of men, the less excuse one has for praising

them for their actions. Yet Skinner says (p. 58), "What we may call the literature of dignity is concerned with preserving due credit. It may oppose advances in technology, including a technology of behavior, because they destroy chances to be admired and a basic analysis because it offers an alternative explanation of behavior for which the individual himself has previously been given credit."

It is an interesting point, like all of Skinner's, yet I remain skeptical. First, Skinner offers very little evidence that "the literature of dignity" has in fact opposed advances in behavioral technology, though logically it ought to have done so. Second, it is not as easy as he thinks to destroy reasons for admiring others or being admired oneself. Presumably, for instance, people have always known that the beauty of a beautiful woman is not her fault. Accordingly, she should get no credit for being beautiful. But we all know that she does damn well get that credit, and usually is ready to accept it too. Or take an example closer to home. As I have said, I believe firmly that every single bit of my behavior has been predestined from ages eternal, and therefore I give intellectual assent to the proposition that, not being an autonomous individual, I should be given no credit for anything I do. Yet I observe that in fact I much enjoy being praised for articles and books that I write, and I believe that the praise (when I get it) makes it more likely that I shall write further articles and books. I believe that the same is true of Skinner himself. That is to say, being given credit is, paradoxically, part of the very determinism which discredits giving credit. In the words of Emerson's poem, "When me ye fly, I am the wings."

On this issue, it is curious that a behavioral psychologist like Skinner should rely so heavily on a purely intellectual and logical argument and so little on observation of actual human behavior.

Let Skinner be very careful in arguing that the literature of human dignity stands in the way of further human achievement. His own type of literature might be more likely to have that effect. If it really persuaded people—which it will not—that they ought not to praise others, since these others are not autonomous individuals and so deserve no praise, he would rob mankind of one of the most powerful of positive reinforcers. But to get achievement, we must have reinforcers, and they are not so plentiful that we can afford to forgo any of them. To quote Mr. Justice Holmes once more, this time his elegant statement of the paradox of behavioral determinisms, "The way in which the inevitable comes to pass is through effort" (*The Holmes-Einstein Letters*, ed. J. B. Peabody [New York: St. Martins Press, 1964], p. 5). But if effort is to be sustained, it requires, by Skinner's own argument, reinforcement.

For the reasons given, I doubt that the freedom and dignity will much impede the systematic application of behavioral principles. What is much more likely to impede it is something Skinner gives little thought to. Again and again he argues that behavior is under the control of contingencies set by its environment. He is quite right to do so, for that insight is the heart of behaviorism. Accordingly, if the environment can be appropriately

arranged, the behavior will be forthcoming. As he says (p. 149), "The outlines of a technology are already clear. An assignment is stated as behavior to be produced or modified, and relevant contingencies are then arranged."

Are then arranged! I like that use of the passive! *Who* does the arranging, and is he *able* to do it? Skinner knows better, but he sometimes talks as if "the environment" were always readily manipulable. Usually by far the most important part of the environment of behavior is the behavior of other men, so that when we try to manipulate the environment to change the behavior, we find that what must be manipulated is still more behavior, and we are right back where we started. That is, we must manipulate the environment of a social relationship between two or more persons, and so on in an infinite regress. The task may easily become expensive.

Skinner hardly gives a thought to the crucial question of cost. He does not appear to realize that one may have developed a technology that is capable of securing particular results but still not apply it, because the cost of applying it is too high—cost in the sense of alternative and rewarding results that must be forgone if it is applied. For instance, he speaks (p. 6) of "making it possible for everyone to be gainfully employed and, as a result, to enjoy a higher standard of living." He implies that this result is not now feasible. I suggest that it is wholly feasible to employ everyone, but that its costs, in inflation for one thing, may be higher than the government and citizens are willing to bear. Again, it may well be feasible to plan an economic system in detail, but the costs of planning may be greater than the results are worth. In the same way, the cost of a systematic behavioral technology may provide the chief obstacle to its application.

As Skinner himself says (p. 150), "The technology has been most successful where behavior can be fairly easily specified and where appropriate contingencies can be constructed—for example, in child care, schools, and the management of retardates and institutionalized psychotics." But note that these are all situations in which the persons who are to apply the technology already have so much control over others that arranging appropriate contingencies is relatively inexpensive. The persons to be controlled are so little in a position to exercise countercontrol that they need not, so to speak, be bought off. Outside of such situations, the costs of applying a sophisticated behavioral psychology *in detail* are apt to become prohibitively expensive, especially if the number of highly trained behavioral psychologists, needed to apply the technology but otherwise unproductive, must be high in proportion to the rest of the population. Then it might be that our old, hit-or-miss, but relatively cheap methods of controlling one another might, on analysis, prove to get us more for our money than the best of behavioral technologies. At any rate, it is a question that Skinner nowhere raises.

Much more likely will be the gradual extension of the kind of control that makes use of existing institutions and envisages gross and only statistically significant results, achieved through altering in some degree the payoffs of a large number of persons. The kind of control that produced the recent devaluation of the dollar is an example. Such controls are just

as much based on the principles of behavioral psychology as any that Skinner proposes, though the persons who employ them may not be aware of the fact. They come quite cheap for the results obtained. And no one seems to raise against them the moral issue that all control is wrong.

Skinner regularly helps to create the fuss that surrounds him by seeming to claim too much for behavioral psychology. He writes (p. 19), "A technology of operant behavior is, as we shall see, already well advanced, and it may prove to be commensurate with our problems." The technology may indeed be on hand, but that does not mean it can easily be applied. At times, Skinner talks too glibly about "the design of a culture," yet he always keeps returning to sobriety (p. 156): "Perhaps we cannot now design a successful culture as a whole, but we can design better practices in a piecemeal fashion." What nervous conservative ever claimed less? It is even pathetic to find him writing (p. 158), "We have the physical, biological, and behavioral technologies needed 'to save ourselves'; the problem is how to get people to use them." What reformer for the last 5,000 years has ever uttered any other complaint? What is so powerful about a behavioral technology that is not powerful enough to get people to use it? If Skinner is right, they would use it if appropriate contingencies in the environment could be arranged. But where is he to place the fulcrum from which the world is to be moved?

Finally, Skinner must ask the inevitable question (p. 103): "If a scientific analysis can tell us how to change behavior, can it tell us what changes to make?" Many people would agree on the changes that ought to be made, if possible, in the behavior of a retarded child—and that is one reason why a behavioral technology can be applied in this case at relatively low cost. But it is not a scientific analysis that brings about the agreement, and there are many cases in which no agreement of any sort is in sight.

In effect, Skinner proposes that "the survival of a culture" (p. 129) be the criterion according to which changes in behavior should be evaluated: those changes should be made which help a culture to survive. Such a criterion appears to make good sense, if only for the reason that men hold many of their values just because these values helped mankind as a species to survive. These values had survival value.

Unhappily for the criterion, it is not as unambiguous as it looks. Skinner to the contrary (p. 129), a culture does not really correspond to a species. Many species have failed to survive, but very few cultures. They are not killed off but are absorbed by, and melted into, other cultures, changing and developing over time. The Roman Empire failed to survive, but much of its culture still forms part of our culture today. The survival of a culture is a rubber criterion.

Suppose, for the sake of argument, that we know what "the survival of a culture" means. Skinner then specifies the behavior that contributes most to the survival of a culture, and therefore the behavior most to be promoted by a behavioral technology (p. 136): "The simple fact is that a culture which *for any reason* induces its members to work for its survival, or the survival of some of its practices, is more likely to survive."

This may be a necessary condition; it is not a sufficient one. For it is not just a question of whether the members work for the survival of their culture, but what they work *at*. Suppose they decided that the best way to ensure the survival of their culture was by destroying all other cultures and worked hard to that end. They might indeed ensure the survival of their culture, but then again they might not, and I doubt that any scientific analysis now existing could demonstrate they were either correct or incorrect in advance of the event. The fact is that we do not know just what practices will ensure the survival of a culture—except a return of all cultures to the level of the hunting-gathering band. We know that cultures can survive at that level, for they did so for hundreds of thousands of years. If we do not return to the hunting band, it will not be because a scientific analysis told us not to.

Skinner even says (p. 135), "Practices which induce the individual to work for the good of others presumably further the survival of others and hence the survival of the culture the others carry." But do they further the survival of the culture that the individual in question carries? I wish I could believe, as a universal generalization, that they did, but I find that I cannot and that I must discriminate. I can think of cases in which the statement holds good, but unhappily I can think of others in which it does not. One of them is the case of shipwrecked mariners adrift in an open boat, short of food. If each works for the good of the others, all will soon starve. If some work against the good of others and kill them, a few may survive and their culture with them. With the present increase in world population, the cultures of the world increasingly resemble the shipwrecked mariners. If some refrain from killing the others, it will not be because a scientific analysis has forbidden it. Consider what else might do so!

In short. Skinner's procedure for deciding what changes in behavior ought to be made turns out to be facile. It fails to take account of the actual difficulties. Yet he has thought to some purpose about the problems of applying a behavioral psychology when most of the opponents of its application have been quite thoughtless. He has asked the right questions and has provided an immense stimulus toward finding better answers than those he proposes himself.

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As Skinner himself has said, "No one goes to the circus to see the average dog jump through a hoop significantly oftener than untrained dogs" (*American Psychologist* 11 [May 1956]: 228). If you want a dog to do something, you train him until he does it on call. That's what the paying customers demand.

In this new book, he asks why we should settle for much less in our

demands on the methods of social management. We face terrifying problems in the world today. We must, he says, contain a population explosion, stave off world famine, prevent a nuclear holocaust, provide housing and transportation, eliminate ghettos, cure disease, and stop pollution. "In short, we need to make vast changes in human behavior" (p. 4). What we need is a technology of behavior. "We could solve our problems quickly enough if we could adjust the growth of the world's population as precisely as we adjust the course of a spaceship, or improve agriculture and industry with some of the confidence with which we accelerate high-energy particles" (p. 5).

The reader who believes with Skinner that we are moving "inexorably" toward "catastrophe" (p. 5) will not gain heart from the pages that follow. We are far—very far—from the technology required. It takes long, hard work to train a single dog. How much more difficult will it be to train mankind—especially since we are not certain of which behavior we really want to produce! Nonetheless, in that way lies our hope. Perhaps with a large enough program of federal grants-in-aid. . . .

But, even if it could be accomplished, would not such training be intolerable—an ultimate totalitarianism? Not at all, says Skinner. We are fully protected by our genetic endowment. This protection operates in two ways. First, we are so constructed that we act to free ourselves from harmful contacts and consequences—from what he calls negative or aversive stimuli or reinforcers. Second, the new behavior that our crises require can be attained only through the use of positive reinforcement. To overcome our crises, men must come to do the right things and not merely to avoid doing the wrong things. It so happens that animals, men included, can be trained to do the right things only if they are given positive reinforcement, only, that is, if they are so trained that the desired behavior leads to positive consequences. When a bit of behavior is followed by a positive consequence, that behavior is more likely to occur again.

To summarize, we are innately so equipped that we try to free ourselves from what is harmful and we can learn to behave only in ways that we will, by innately given standards, "want" to repeat. We cannot be other than true to our genetic endowment. It guarantees that we cannot be trained to behave in ways that are harmful (pp. 103–26, 128–29).

We need, so Skinner urges, to distinguish between (*a*) the comprehensive control of behavior and (*b*) totalitarianism. There is nothing wrong with control itself. It is the means, and the only one, by which we acquire our most valued patterns of behavior. The problem with totalitarianism is not that it entails control and is comprehensive, but that the control exercised is aversive.

What about human freedom and dignity, you ask? What about responsibility and goodness? No problem, Skinner answers, providing you will use these words in the way that makes sense. Unfortunately, such usage is not common among scientists, scholars, or ordinary men. When they talk about man's freedom, they mean that his behavior is uncaused (p. 19). That meaning is patently untrue. The tenable meaning of freedom

is success in freeing oneself from negative reinforcement (pp. 26, 44). By the same token, dignity is the ability to attain positive reinforcement (pp. 44–59). Both are maximized in the new world Skinner designs. By his definition of that world, it cannot be otherwise.

As for responsibility and goodness—as commonly defined—no one in the new world would want or need them. They refer to a man's behaving well despite the absence of positive reinforcement that is obviously sufficient to explain it. Where such reinforcement exists, "no one needs goodness" (p. 67).

Skinner insists that man's very effort to believe that his behavior is free—in the meaning of "uncaused" behavior—has helped to bring us to the precipice. It has worked against our developing the technology of behavior that we so desperately need. To place a high value on our being responsible and good is also to work against ourselves, because these words are meaningful only if we live without sufficient positive reinforcements To value these words is to preserve a social order that depends primarily upon punishment, that is, upon the application of aversive reinforcements or, what is also, if differently, aversive, the removal of positive reinforcements, this in an effort to remove undesired behaviors. One learns from punishment how to avoid it—not how to behave well. When, Skinner asks, will we stop trying to do the impossible—leaving it up to each person to learn how to behave without positive reinforcement?

I have lifted out what seems central and distinctive in Skinner's new presentation. I do this because it needs to be examined for its own qualities and, to the extent one can, apart from his position on the study of behavior. That position has frequently been evaluated. It embodies, I think, great powers over an important if narrow domain.

One can judge what seems newest in the present book even apart from the rest of his work. I take what is new to be his extended elaboration—and application to the ends of social action—of a thesis he has enunciated before: the thesis that what is positively reinforcing is right for the animal whose behavior is controlled by that reinforcement. He suggests that this thesis was forced upon him (1956, p. 233):

> In my early experimental days [the one idea in my life] was a frenzied, selfish desire to dominate. I remember the rage I used to feel when a prediction went awry. I could have shouted at the subjects of my experiments, "Behave, damn you, behave as you ought!" Eventually I realized that the subjects were always right They always behaved as they ought . . . What a strange discovery for a would-be tyrant, that the only effective technique of control is unselfish.

We need not trouble ourselves about possible confusions here in Skinner's use of "is" and "ought" or the limitations of his philosophical position or the adequacy of his sociology. He himself tells us that his recommendations turn on a simpler point. For those recommendations to be workable, it must be the case that people will find positively reinforcing that which will save them. And he agrees that this may not prove to be true. What

people find reinforcing is a *joint* product of their biological capacities and the options that their environment provides. That environment may reward indolence rather than effort, deceit rather than honesty, withdrawal rather than courage, mindless consumption rather than the wise conservation of resources, aggression rather than affection, and so on. Skinner himself will not call such positive reinforcements right or good or, at the least, will not say that they are as right or good as he wants (nor does he draw the inescapable conclusion that our genetic endowment is not sufficient in itself to produce or protect the good). He agrees that every living man may be hopelessly incapacitated to be reinforced by what he should:

> The problem is to design a world which will be liked not by people as they now are but by those who live in it. "I wouldn't like it" is the complaint of the individualist who puts forth his own susceptibilities to reinforcement as established values. A world that would be liked by contemporary people would perpetuate the status quo. It would be liked because people have been taught to like it, and for reasons which do not always bear scrutiny. A better world will be liked by those who live in it because it has been designed with an eye to what is, or can be, most reinforcing [P. 164]

Skinner does not suggest who would be competent to decide what "is, or can be . . . right," or the means by which that decision would be made or validated, or the means by which recalcitrant people would be brought to the way they surely ought to go. That being so, the paying customers will behave as they ought to and will keep their money in their pockets.

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Like any good social scientist, B. F. Skinner knows that feeling and behavior are shaped by conditions external to the organism, and he has spent much of his professional life attempting to discover the precise sets of conditions ("contingencies of reinforcement") sufficient to produce the behavior society regards as desirable, and the by-products of that behavior —feelings and other subjective states. He is now at the point where he thinks he knows how to accomplish this. Although he is uncharacteristically reticent about exactly what they are, the technologies of behavior control are available, he says, to create the kind of social environment (and hence the kind of people) we want. Standing in the way of the effective utilization of these technologies are a couple of widely shared positive sentiments: "freedom" and "dignity," a pair of words with which society rewards and honors (i.e., positively reinforces) those people who seem successfully to resist or transcend the conspicuous constraints which attempt to control behavior.

Skinner is irritated and impatient with this situation. Rather like a Mad Scientist from some old low-budget movie, prevented by the ethics

and conventions of his profession from carrying on his Weird Experiments to save mankind from itself, he is driven to attack its most sacred cows. "Autonomous man," then, is the villain of this book and "the literature of freedom and dignity" which supports him in his delusion that it is he alone. the "inner man" (and not environmental contingencies), who deserves the credit and the blame for his good or bad behavior.

Given Skinner's disdain of freedom and dignity and his belief that persuasion ("the attempt to change minds") is merely a weak positive reinforcer, the most obvious logical question that occurs is why he bothered to write the book at all—a question raised by at least a few earlier reviewers. There is an answer, although it is a disquieting one: Skinner says that persuasion is effective only if there is already some predisposition to behave and that "we persuade someone by making a situation more favorable for action." By giving his explicit expert sanction for programs of behavior control to those in high places (the president, the attorney general), the book supports those who are already favorably predisposed toward behavior control. The book is obviously addressed to those Established enough to be able to make authoritative decisions (the royalist "we" is the book's most pervasive rhetorical device, as in, "What we need is a technology of behavior," etc.), and Mr. Nixon is exactly the man who, with his lust for perfect clarity—if not for logical consistency—could affirm freedom while acting to institute behavior control.

Why would Skinner be interested in persuading men in high places to initiate effective programs of behavior control? Under his system, he says, it would be environmental contingencies (not the inner man) which got the credit for the good behavior of individuals. But in a human society, this could mean only that the *designers* of the system of environmental controls (i.e., Skinnerians) would get the credit; and the fact that Skinner does not make public in this book *how* behavior is to be controlled suggests both why the book is addressed to an elite and why there is reason to be apprehensive about it.

These, of course, are political apprehensions, and it is remarkable that in a book about redesigning a culture in ways that would control behavior more benevolently—a project which, as Skinner says, involves "altering the conditions under which men live"—there is hardly a word about power or political struggle. It is not likely that this is an accidental omission. Struggles over the alteration of the conditions under which people live is exactly what politics is about, and powerful people do not often yield to others the right to alter the conditions of their lives. Powerless people are not so fortunate, and in Skinner's system, in which status would be explicitly a function of the specially advantageous environments to which one had been exposed (it does not seem to occur to him that in fact, if not in ideology, it already is), the underadvantaged would apparently be denied even the small compensation of the myth of their dignity and autonomy.

When questioned or interviewed, Skinner typically pooh-poohs the felt apprehensions about the totalitarian, 1984-ish implications of his programs

for behavior control with reassuring phrases like, "It is not the benevolence of a controller but the conditions under which he controls benevolently which must be examined." But he mentions only two contingencies: (1) that all control is reciprocal and (2) that the controller himself must be controlled by his system of controls. Neither is in fact very reassuring. Social control is no doubt reciprocal, but the reciprocities are seldom equitable, or there would be no problem of exploitation. Moreover, democracy (a system in which legislators live under their own laws) has never been any insurance against injustice.

If, as an essay in persuasion, Skinner's book begs some important logical questions and is frightening with its answers to others, are there other grounds on which "we" (ordinary people, that is) might find him persuasive? Skinner, of course, is a utopian, and if his plans for redesigning the culture contained some morally ennobling vision of the future, "we" might be persuaded in spite of "our" apprehensions. But Skinner is a utopian without a moral vision, and therein lies a problem. Listen to him: "Our task is not to encourage moral struggle or to build or demonstrate inner virtues. It is to make life less punishing." Life is made less punishing in Skinner's faith by moving away from reliance on primitive negative reinforcements (in which people accede to controls for fear of the consequences of not acceding: "Do it or I'll kill you") to greater reliance on positive reinforcements (which move people to behave in ways that are rewarded: "Try it, you'll like it"). Now there is no question that seduction is less punishing than rape, but as an ennobling vision, it sort of lacks something. Skinner says not a word about love.

Skinner is not merely for the reduction of environmentally caused pain, he is also for greater efficiency in the design of a culture in which behavior likely to be punished seldom or never occurs. "We" have already designed such a world for "babies, retardates, or psychotics, and if it could be done for everyone, much time and energy would be saved." The efficient husbanding of time and energy is a quality much admired in managers and administrators, but *for what* is a question that groups have been killing each other over for a very long time, and if the best answer that Skinner can come up with is to render the rest of us as painless as babies, retardates, and psychotics, the answer is again less than ennobling; in any case, I should prefer opium.

Skinner has trouble with "for what?" questions because he does not believe in the possibility of moral choice. For him, statements of value preferences are all reducible to "if, then" propositions. "Thou shalt not kill" *means* if you do not want to go to jail, do not kill anybody. The closest he comes to any statement of faith is in his affirmation of a kind of Darwinian evolutionism in which "survival is the only value according to which a culture is eventually to be judged." But even this is far from persuasive because Skinner has nothing to say about which specific practices do or do not have survival value, and from the perspective of deliberate behavior control, a survival ethic is less than useless because adaptive behavior is often unintentional and intentional survival behavior

often has the opposite consequences. At a time of ecological crisis, it is surprising that we even need to be reminded of those facts.

Moreover, cultures do not induce their members to work for the culture's survival, as Skinner suggests; they induce them to maintain specific practices and to change others (which is not the same thing), to affirm its sacred symbols and to resist their desecration. But in a complex society, the objects of maintenance and change, affirmation and desecration, are themselves matters of controversy; and if, as Skinner says, a culture is a set of contingencies of reinforcement, all people are always working for the preservation of some and the destruction of others, and the adaptive or maladaptive character of the consequences is as much a matter of politics (in which "history is the only judge") as it is of science. It is, indeed, one of those areas of public life in which it is difficult to make the separation.

Beyond Freedom and Dignity, then, is a book that is persuasive neither at the level of logic nor as a visionary appeal for a morally ennobling future. Still, Skinner's arguments warrant attention if for no other reason than that they are not apparently designed to win him popular acclaim Imagine the effrontery: arguing against freedom and dignity! Unlike those unemployed aging actresses righteously announcing on television talk shows that rather than work nude they would prefer not to work at all, Skinner actually risks something. But the book is disappointing. It reminds me of a remark made in Wright Morris's *Love among the Cannibals*, a novel about two Hollywood songwriters down on their luck and trying to write a hit. "It doesn't have to be a good song," one of them says, "just one that *reminds* you of a good song." *Beyond Freedom and Dignity* reminds you of a good book.

Book Reviews

Law, Society, and Industrial Justice. By Philip Selznick with the collaboration of Philippe Nonet and Howard M. Vollmer. New York: Russell Sage Foundation, 1969. Pp. viii+282. $7.00.

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At this time the sociology of law is quietly drifting toward a conflict between two schools of thought. The first is pragmatic and sometimes normative; the second strives for detachment and neutrality. The first moves freely between fact and value, seeing a rigid separation as undesirable if not impossible; the second clings to this separation and shows no sign of weakening. The first finds the second naïve, but is in turn criticized as confused. Call the first a natural-law approach, the second a positivist approach. However familiar and tiresome this debate may be in its jurisprudential version as well as in other areas of social science, it is a sign of life in the sociology of law, a field otherwise undisturbed and undistinguished by theoretical discussion. What is more, it would be irresponsible to offer an assessment of Philip Selznick's *Law, Society, and Industrial Justice* without explicitly taking sides in this controversy. Selznick's study is undoubtedly the most erudite and imaginative example of the natural-law approach to appear. Yet as an uncompromising adherent of the positivist approach I must locate his contribution in what Roscoe Pound called "sociological jurisprudence" rather than in the sociology of law. After reviewing the argument of Selznick's book, I shall return to the somewhat delicate but critical question of its intellectual identity.

Selznick argues that legal ideals previously monitoring the exercise of public authority can now be appropriately extended to the regulation of private authority, particularly to that lodged in the industrial employment relationship. This extension of the rule of law, he claims, is a tendency of moral evolution. He concentrates on the very edge of this evolutionary trend, showing how legal doctrine and the social characteristics of industrial employment are dovetailing toward a higher legality, specifically toward an expansion of the legal concept of governance and a recognition that governance is found in industrial as well as political life. Hence, Selznick looks forward to an application of "due process" standards of governance to private associations. He concerns himself with the capacity of private organizations to "establish justice," seeing that capacity as a function of the social dynamics of the organizations themselves (p. 34). Therefore, the legal sociologist finds himself in a position to justify legal reform and chart the course of legal change: "To extend the rule of law is to build it firmly into the life of society, to make the master ideal of legality a true governor of official conduct. . . . In contributing to that effort, students of law and society are confronted with a special intellectual

problem: how to bring legal ideals to the 'private' sector of community life. . . . The issue is: Can we justify, within the framework of legal theory, the application to private organizations of principles hitherto restricted to public government? That question sets the theme of our study" (p. 35).

The analysis begins with an overview and critique of the law relevant to the social system of industry—the law of corporations, of contracts, and of property. In each case Selznick finds that these legal frameworks "fail to grasp the reality of association" (p. 63). Property law, for instance, is thing centered, individual centered, and domination centered, while it ignores the power relations between persons that are created by property. Selznick protests that property law views the world as if Karl Marx never existed. In short, today's law of associations operates with outmoded concepts unenlightened about industry as a social arena and unequipped to appreciate the nature of membership and authority in the employment setting.

But Selznick is not satisfied merely to expose the law's incompetence from the standpoint of a sociologist; he also looks for moral pressures originating within the industrial bureaucracy. He finds a "strain toward legality" that is "natural" to bureaucracy (p. 93). Arbitrariness is inconsistent with the very constitution of bureaucratic organization. Drawing on Max Weber's work, Selznick notes that bureaucratic authority is "legal" because its legitimacy is warranted by rational principles and because it is formally committed to an order of rules, with every bureaucratic decision open to criticism in the light of established procedure and explicit purpose. Again, the source of these attributes is internal and the dynamic they create "calls forth the ideals of legality" (p. 81). At this stage of his argument Selznick also presents interview data from a 1958 study of 44 personnel directors, another way of peering inside the bureaucratic system. Examining their conceptions of fairness, he found a preference for limiting management's jurisdiction over employees and for justifying every extension of this jurisdiction. Hence, we see that some of the managers as well as the system of bureaucracy aspire for the rule of law.

After tracing the historical development of the law of employment from its beginnings in the master-servant framework, to employer-dictated contract, to the collective contract based upon bargaining between labor and management, Selznick pauses to consider the character and contribution of arbitration as a system of industrial justice. He reveals how "creative arbitration" in dispute settlement is another empirical element thrusting industry toward a regime of legality, thereby adding another link to his evolutionary argument.

One is struck by the enormous range of material Selznick covers in making his case. He even includes a chapter reporting employee attitudes about industrial justice, based upon a 1961 survey of almost 2,000 men in eight organizations. Here, for example, we learn that employees tend to oppose the use of seniority as a processing principle—a principle often thought

to be progressive and prolabor. There is thus a "significant gap between the subjective preferences of many employees and the rules by which they are governed" (p. 210). One at first wonders why Selznick includes interview data in a legal study. What difference does it make? In fact, it makes a great deal of difference in a natural-law approach. From this standpoint men's expectations and hopes are often part of the law that is becoming. If they are not, they at least indicate the law that ought to be, itself a major force ultimately defining the law that is. Natural law involves a conception of what Lon Fuller called "the law in quest of itself." Attitudes about justice, then, bring Selznick closer to emergent law and the law that is possible, closer to the point where the "is" and the "ought" meet.

The last major generalization in the book is that there has been an "erosion of public purpose" in the law of employment (p. 240). Because this law has been administered passively, with reliance on the private-law system of citizen complaint to select its problems, it has been little inspired by larger democratic ideals and values. Justice in employment relations has not been sought for its own sake as a public policy. Fair employment practices, for instance, have become a tangential concern of a larger civil rights program, not an end in themselves. The quality of governance in industry is hardly a focus of law at all. And yet the evidence seems overwhelming that industry is a "receptive institutional setting" prepared for the advent of legal change (p. 243; italics omitted). With his many diverse empirical materials—from case law to attitudes to legal administration—Selznick tries to show where the law has come from, where it is going, and where it could go.

While most of his study emphasizes the social system of employment as a component of industrial law in action, in the final chapter Selznick considers the conceptual system of public law as a resource and solution for industry. As he puts it, "We are interested in the posture of the legal system, its conceptual readiness to seize what we take to be an emergent historical opportunity" (p. 243). He finds the answer in the public-law concept of due process. Due process is a legal framework geared to the limitation of arbitrary power and is basic to the legal control of government officials. It is the heart of the law of governance. In brief, Selznick reasons that this law of governance properly refers to a social function involving the rule of some men by others, not to a particular institution, that is, not to public government alone. Private organizations have government as well. It follows that due-process constraints apply to the governance inherent in the employment relationship and to any other form of governance in private life. The book ends on this conclusion.

What is unique and significant in Selznick's study is his preoccupation with the coming into being of law. He tries to bring order to what he calls "incipient law" and "inchoate law" and more generally to the doctrinal evolution of law. Sociologists often shy away from the law that is unclear or ambiguous, the developing law or the law in dissolution. They look instead to the predictable law, the law in fact. Selznick, by contrast, not only seeks to specify where the law is going; he also tells us where the law

should go if it holds to its own mission. He interprets the law. In my view, however, this is where Selznick leaves sociology and enters jurisprudence.

Selznick's analysis of the law follows along two axes, each pertaining to whether the law is true to itself. The first asks about the law's inner order, its latent values or ideals. These are the ideals of the legal system itself and of its participants; they may be delineated and applied to the evaluation of the law independently of the sociologist's own preferences, says Selznick. The second axis of his analysis examines the law's implicit conception of the world as it is. Every normative system implies a vision of reality, indeed, a sociology. As Selznick notes, "Norms derive, not from the ideals of the system alone, but also from knowledge of what men and institutions are like. Only thus can we know what norms are required to fulfill the ideal" (p. 28). Here the sociologist evaluates the law's social knowledge of its own jurisdiction. The law, then, is a system of both ideals and intelligence, each central to a sociological study of the law's effectiveness. Let us consider more closely the place of these elements in a natural-law approach.

How does Selznick explain legal change? While he exerts much effort simply trying to catch the pattern of history, and while he posits moving power in the attitudes and reactions of citizens to empirical legal life, at bottom the crucial stimulus to change comes from within the legal process itself. Law changes to meet the demands of justice, and justice is defined, paradoxically, by the latent values of the law itself. Conceptions of justice, moreover, are ever changing as the law confronts a changing world. I find, however, that serious problems arise in the application of this strategy, since what is just in the inner order of the law is far from apparent. If the law's latent conception of justice is even remotely as elusive as it seems, an approach to legal change that attributes explanatory power to this conception may leave something to be desired. What is more, to my mind the law's underlying mission cannot be discovered through the scientific method. Science is unable to divine what is just from the law's own point of view, since this is a moral and political question. Science may tell us what others define as just or right or good, but it alone cannot give us a normatively valid definition of the just. Even if Selznick is correct in observing that justice must be defined within the ongoing legal process, we cannot look at the question from within the process and at the same time remain neutral. Selznick attempted to make a kind of value-free value judgment. He attempted the impossible. Sociology can never discover the law's meaning from a legal standpoint; that is inescapably a normative question. When Selznick finds the law of employment inadequate to the demands of justice and asserts that the law is conceptually ready to extend due-process protections to private settings such as industry, he is far beyond a sociological perspective. He is an advocate. His conclusion is logically independent of his sociological investigation, and it has no empirical grounding. It is saturated with ideology and evaluation and interest. There is no other kind of legal interpretation.

Recall that Selznick's second major focus is the law's implicit image

of the social world. Again and again his argument emphasizes how legal ideas fail to recognize the sociological reality within which legal acts occur. Legal theory abstracts these acts from their social context and treats them as if they occur in a vacuum.

For this reason we may criticize the law and suggest how it might take account of the reality sociology reveals. Thus, for instance, the reality of employment involves power and governance, but the law operates as if power and governance were found in public administration alone. If only the law were realistic the protections of due process would be extended to employees vis-à-vis management. This is much like the argument so often made by sociologists that if the criminal law were only to recognize the sociological and psychological reality of crime it would have to discard the traditional framework that casts the criminal as a free agent who wills his crime and is held responsible as if he could have done otherwise. Similarly it is commonly noted that contract law proceeds as if differences in bargaining power did not influence the nature of contracts. But I wonder why law should present an accurate sociological analysis of its jurisdiction. Law is not science and was never meant to be. Law is normative. A scientific critique of law therefore makes no more legal sense than a scientific critique of religion makes theological sense. But surely Selznick does not think that the social nature of industrial employment is unknown to lawmakers and judges. Surely he means something else. He must mean to be making a *case*, a case that employees should have more legal advantages than they now enjoy. That is well and good, but sociology alone will never be enough to compel a normative conclusion of that kind. Selznick, however, tries to reason from his sociological description to the needs of the law, to what he calls "legal implications." But science cannot judge the law. Sociologically, the path of the law is wherever it goes, and no sociologist, as such, can point its proper direction.

Law, Society, and Industrial Justice is an impressive contribution to sociological jurisprudence, a field of legal scholarship concerned with assessing the law in the light of the social system's functional requirements. Accordingly Selznick interprets the law's own aspirations, just as a social critic might interpret a society's cultural values and draw policy implications with respect to how those values could be realized. In both instances the interpretation necessitates social partisanship, since the meaning attributed to the law or to cultural values generally implies differential costs and benefits across the society's membership. Selznick, for example, interprets the law in the interests of labor. His is a liberal jurisprudence. This is also apparent when he assesses the legal requirements of the employment relationship considered as a sociological entity, apart from the question of the law's meaning. I am not saying Selznick is biased; I am saying that he, like any jurisprude, is normative and political at the core of his analysis. Even if we accept, *arguendo*, that legal criticism can be done without partisanship but with the interests of the whole society alone at stake, it still must be normative and political. Such would be a form of social eudaemonism, an ethic holding that what is right is what advances the well-

being of the whole society. The needs of the whole take priority over all other interests. This would be functional jurisprudence with a vengeance, a kind of sociological totalitarianism. It is hard to tell just what kind of jurisprudence Selznick favors, and that is precisely because he is not explicit about his legal philosophy. Instead he presents his study with an air of innocence, as if it were sociology and nothing more.

In the study of law it is still important to be clear about what sociology is and what it can do. Sociology cannot tell us whether the state should hold employers to due-process standards in their authoritative dealings with employees, and a book that seeks to "justify" that reform is not sociology. Selznick's argument is part of a larger movement toward an ever greater involvement of the state and law in the private affairs of the citizenry. The book may even add to the progress of that evolutionary trend, a trend Lon Fuller once described as "creeping legalism." Selznick both celebrates this trend and applies sociology to its advance. One could as well apply sociology to its reversal on the ground that government intervention is dangerous, an evil to be avoided at all costs, even if intervention would appear to be benevolent. To ask the law's help is to widen its already vast jurisdiction. I do not want to make that antilegal argument at this time, but mention it only to dramatize the distinctively political character of Selznick's study. The use of sociology to sponsor jurisprudential premises is misleading if it is not deceptive. The search for "natural law," for value in the world of fact, is hopeless. *Law, Society, and Industrial Justice* is a very fine, even extraordinary piece of legal scholarship. It displays much craftsmanship, depth of learning, and creativity. It is elegant in style and graceful in presentation. Every legal sociologist should read it knowing exactly what it is—a good example of what the sociology of law is not. To say this about a book by Philip Selznick seems almost outrageous, and it must seem ungrateful as well. But at the beginning I gave warning of my positivist commitment.

Knowledge from What? Theories and Methods in Social Research. By Derek L. Phillips. Chicago: Rand McNally & Co. Pp. xx+204. $3.95 (paper).

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The title of the book reflects the author's critical examination of the measurement processes in social research which produce sociological knowledge. Derek Phillips's discontent with existing measurement techniques has a biographical origin in his own expert survey research in mental health. Phillips found that the survey does not systematically consider the sources and consequences of the factors operating in the data collection process. This realization led him to consider the validity of

existing measurement instruments, to recommend improvements, and suggest alternative sociological conceptions.

To Phillips, a fundamental irony in sociology is that it professes to be the study of human interaction, yet the most frequently used sociological instrument prohibits direct observation of this phenomenon. The vast majority of sociological studies (90% in *ASR* between 1962 and 1969; 92% in *AJS* and *ASR* in 1965–66) utilize the survey which requires respondents to report on their behavior or feelings.

Although measures may be invalidated by showing they correlate highly with a simpler variable or correlate poorly with an independent measure, few survey instruments are examined for systematic invalidity and bias. When studies of respondents' personal characteristics (e.g., age, marital status) and their personal activities (such as voting, deviant behavior) have been checked against official records, a low correlation between self-reports and official records has been found. Despite sociology's concern for objectivity and control, the potential source of interviewer-respondent interactional contamination in data collection (e.g., the way questions are read, the race and sex of researcher) located in psychological lab studies have not been considered as variables in survey research. Phillips says the validity of survey results cannot be assured until the influence of situational factors is proven to be unsystematic.

Phillips suggests improvements and recommends alternative strategies based on his critique of current measurement practices. He urges more validation studies, the inclusion of interviewer and respondent factors in research design, and recommends the simultaneous use of many data-gathering techniques. Although Phillips says the respondents' meaning of the research situation should be a factor, he overlooks the necessity of examining their interpretations of stimulus items and instructions. The recent work of ethnomethodologists (Garfinkel, Cicourel, and his students) have shown the necessity of examining the assumption that the respondent and researcher share the meaning of interview items as a potential source of bias. The limited space which Phillips devotes to the problems that language and meaning pose for survey research is one of the few shortcomings of the book.

He dismisses "non-reactive" or "unobtrusive measures" as alternatives to surveys because they avoid possible interactional contamination at the price of extreme behaviorism. Removing the observer from interaction with the respondent forces the researcher to interpret behavior out of ignorance. Researchers always view respondents' behavior against background assumptions. Because the researcher's knowledge about social behavior is dependent upon his research methods, which in turn, are dependent upon the researcher's social knowledge (p. 53), the more the researcher knows about the everyday life of his subjects, the more confident he can be of the observations he makes.

Phillips recommends more direct study of action in everyday life, which leads him to consider the merits of participant observation. He counters the criticism that fieldwork is subjective by saying all research is sub-

jective in that action produced by a person is interpreted by a person (p. 129). Because all research is interaction, Phillips feels sociology should be studying the interpretive sense-making and decision-making processes which are fundamental activities in everyday life and sociological research, with the goal of understanding this process, not correlating facts external to the member and observer.

Phillips proposes ideal type analysis as an alternative model for sociological theory, which causes him trouble over issues of theory construction, validation, and explanation. Ideal types have been proposed as alternatives of current forms of theorizing before. Phillips does not counter the criticisms these suggestions have encountered (e.g., Paul F. Lazarsfeld, "Philosophy of Science and Empirical Social Research," in *Logic, Methodology and Philosophy of Science,* ed. Ernst Nagel et al. [Stanford, Calif: Stanford University Press, 1962]); he says explanation and intersubjective verifiability will be achieved when the researcher can construct the rules by which action is followed such that another person put in a similar situation would have similar experiences (pp. 161–62). Sociologists "are able to understand people's behavior if they understand their motives, goals, choices and plans" (p. 160). By defining "understanding" as "knowledge of internal states" Phillips has undone the thread of his argument, for as he has said, the survey is the most appropriate instrument to make internal states amenable to study (p. 100). But the sociological topic is not motives and attitudes per se, but the actualization and interpretation of action in everyday social situations.

Phillips's suggestion to define explanation as rule following is weak on three counts. (1) The ethnomethodologist has shown normative rules are incomplete in that they do not tell rule followers how to follow them: "interpretive procedures" are needed to understand surface rules. (2) "Cognitive anthropologists" attempt to specify the cultural rules necessary for interaction. Phillips's discussion of rule following would have benefited from consideration of the literature in that field (e.g., Tylor, *Cognitive Anthropology;* Hymes, *Language in Culture and Society*). (3) To say "X did Y because they are friends" (p. 162) is to say X followed a rule. But Mannheim, on "The Interpretation of Weltanschauung" (*Essays on the Sociology of Knowledge* [London: Routledge & Kegan Paul, 1952]), has shown the interpretation of action can take place on at least three levels, depending on the observer's familiarity with the actor. Phillips does not provide criteria by which decisions about which rule is being followed can be made.

Phillips's recommendations for the alternative to survey research is underdeveloped in that he devotes sentences to topics that require paragraphs and pages. (Only 50 of 200 pages are devoted to "the alternative.") Perhaps this disproportionate coverage reflects the state of the published art of interpretive sociology. Perhaps we must wait for the further publication of ethnomethodological studies to flesh out the interpretive sociological model.

Despite the abbreviated treatment of interpretive sociology, Phillips

has contributed a rich and substantial document to the discussion of measurement. The book should be required reading in methods courses because he has presented a concise and readable portrayal of the existing rationale for survey research, an excellent critique of this technique, and an adequate presentation of alternative sociological conceptions.

Multivariate Model Building: The Validation of a Search Strategy. By John A. Sonquist. Ann Arbor: Institute for Social Research, University of Michigan, 1970. Pp. xiii+244. $7.00 (cloth); $5.00 (paper).

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When researchers learn of new techniques or computer programs for analyzing social surveys, too often they are unable to find any careful investigations of the ways the new tools respond to the properties frequently encountered in nonexperimental data, such as multicollinearity, noise, nonnormality, small sample size, and complicated causal structure. If at all, it is only after a technique has been widely used over a long period, or has been the basis of a controversial substantive finding, that important guidelines and caveats for its use come to light.

Multivariate Model Building is an important exception to this pattern. In this book, Sonquist undertakes the investigation of the properties and usefulness of the Automatic Interaction Detection technique (AID), which he helped to develop a few years earlier. The major guidelines for the use of AID are derived from an analysis of hypothetical data generated from three underlying models, one without any interaction among the independent variables and two with interaction. Data from these models with different degrees of skewness in the variables are analyzed by both the AID technique and the Multiple Classification Analysis method. Conclusions are drawn from the way each method reveals the true underlying models and relative importance of each predictor used to generate the data.

The AID is a stepwise program which operates on a continuous dependent variable and categoric independent variables. Under the AID algorithm, at each successive step that dichotomous split on any independent variable is found which gives the lowest within group sum of squares for the dependent variable for any other split of the predictors. After each such selected dichotomization, the resulting subgroup with the largest within group sum of squared deviations is then eligible for splitting by the same search strategy. The sequence of splits continues if the eligible groups have at least the number of cases and within group sum of squared deviations specified by parameter cards designated by the program user. In the end, what results is output which can be arranged as a tree of successive subdivisions of the population on categories of the independent variables.

The appearance of the AID tree structure is shown to be an important criterion for deciding whether there are certain kinds of interactions among the independent variables. In the absence of interaction in an additive model, a symmetric tree results where each branch is composed of subgroups based on the same selected independent variables. When certain interactions are present in the model, the predictors do not behave similarly in the various branches of the tree. In interactive models where effects are cumulative or substitutive, at each split a subgroup is identified which is homogeneous to the dependent variable and will not be split any further in subsequent applications of the AID algorithm. The resulting tree structure has only one branch which grows along single subbranches, rather than symmetric splits in every subgroup in the final additive tree

While the AID technique was found to identify selected interaction patterns, it was not successful in directing the analyst to clear conclusions about the relative importance of the various predictor variables. The order of variables in the splitting process was not an adequate indicator of a variables importance. Like other stepwise routines, a great deal of attention to intermediate output was required, without always clearing up points of confusion about irrelevant correlates of powerful effects or the primary explanatory variables.

The author concludes that correct unambiguous conclusions about the relative importance of predictor variables are arrived at more successfully with methods such as Multiple Classification Analysis, which present net amounts of variance explained by individual variables in a single analysis of the complete model. Of course, the model analyzed in this way must correctly reflect any interactions among well-conceptualized variables It is in setting up the correct model that AID has its major use.

In the end, a research strategy is proposed which involves the joint use of the AID technique and Multiple Classification Analysis. AID is used to check on peculiarities of scaling and then to scan for interactions among independent variables. With the results of the AID investigations, new indices are to be constructed to provide interaction terms for subsequent Multiple Classification Analysis (or multiple regressions). Many important insights and details are given on how to go through these individual steps.

In the earlier chapters, Sonquist reminds us that knowledge of interactions in social data is often not merely a refinement in a substantive argument expressed first in additive terms, but is frequently the basis for a very different causal story. With a series of examples from sociological studies and a classification of types of interactions, he argues that the routine search for interactions should occur early in empirical studies to develop theory, rather than as an afterthought late in the research. The strategies suggested from his analyses of the properties of the AID technique give researchers a practical approach for following this dictum. As the author suggests, further work is needed to demonstrate the viability of AID for models with intercorrelated predictors which are typical in nonexperimental studies and with more complicated causal structures. As

an initial validation, this book is an important service to researchers who wish to consider the AID program in their empirical work.

Le littéraire et le social: Eléments pour une sociologie de la littérature. Edited by Robert Escarpit. Paris: Flammarion, 1970. Pp. 315. Fr. 7 (paper).

The Sociology of Art and Literature: A Reader. Edited by Milton C. Albrecht, James H. Barnett, and Mason Griff. New York: Praeger Publishers, 1971. Pp. xiv+752. $15.00.

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The sociology of art and literature has long floundered between the Scylla of grandiose, vague, and unprovable generalities and the Charybdis of data gathering to no apparent end beyond that of description. What the field needs, and what it has little of, is more solid, substantive work that will steer a course between the two. Here are two books which ought to stimulate interest to do just that. In these days of cultural crisis in and out of the classroom, sociologists might do well to look to literature and the arts for new perspectives. As the first comprehensive collection of work in this area, *The Sociology of Art and Literature* will prove extremely useful in the too few courses on the sociology of art or literature that are given on American campuses (I have used it in such a course). It may even prompt sociologists to offer more such courses or introduce these and other materials into courses on the sociology of knowledge (see the article by A. Kern, pp. 549–56); small-group theory (see S. Forsyth and P. Kalenda on the organization of a ballet company, pp. 221–55); the mass media (see K. Lang, "Mass, Class and the Reviewer," pp. 455–68); or mass communications and mass culture generally (see A. Toffler, "The Culture Consumers," pp. 483–98; J. Bensman and I. Gerver, "Art and Mass Society," pp. 660–68).

If these two works point up the underdevelopment of the sociology of art as a whole, they also illustrate the considerable differences between what one might call the American eclectic approach and the more directed, intense orientation that certain European scholars bring to the study of art and its multiple ties to the society in which it is created, diffused, and consumed.

The Sociology of Art and Literature demonstrates to perfection the range of American eclecticism. Sociologists account for a minority of the contributors, who include art historians, art and literary critics, an artist, anthropologists, philosophers, historians, and free-lance critics. Such diversity means that while all of the articles raise matters of potential concern to sociologists, many, perhaps most, focus on the particular subject rather than on analytical issues. Reference to social structural variables

is apt to be implicit instead of explicit. One article at least expressly eschews "scientific terminology" (p. 195).

For this reason, the majority of articles do not lie within the mainstream of sociology (or even any of its tributaries) that is close to work which might provide points of reference and a more general theoretical framework. (The principal exceptions to this stricture are Milton Albrecht's introductory essay, "Art as an Institution," reprinted from the *ASR*, the articles in the section on methodology, and James Barnett's brief review, reprinted from *Sociology Today*.)

The volume is divided into six sections, each of which has a brief introduction and a commented supplementary bibliography. Part 1, "Forms and Styles," ranges from A. L. Kroeber's general "Style in the Fine Arts" to a detailed analysis of architecture of the Christian basilica and its correspondence to the theological hierarchy of Christianity by the art historian, F. van der Meer. Part 2, "Artists," covers "Socialization and Careers" as well as "Social Position and Roles." Notable here is Mason Griff, "The Recruitment and Socialization of Artists" (from the *International Encyclopedia of the Social Sciences*) and the study by Anselm Strauss of the Chicago Art Institute School.

Part 3, "Distribution and Reward Systems," includes Harold Rosenberg's lively views on the vagaries of "The Art Establishment" and studies of the effects of patronage in the Middle Ages and Renaissance. In part 4, "Tastemakers and Publics," two articles coauthored by Bernard Rosenberg and Norris Fliegel tackle the organization of the art world (museums, critics, agents). Alvin Toffler's assessment of the potential and actual positive contribution of a mass public for the arts should give pause to the more truculent of the "mass culture" critics, who typically concentrate more on the (real and fancied) plight of the artist rather than the attributes of the public (the concern voiced by John Dewey and Kenneth Clark, and Renato Poggioli's appraisal of artists' penchant for self-pity and ostentatious alienation, all in part 6).

Part 5, "Methodology," presents a number of prolegomena to the study of art and society, plus some caveats of what not to do. Here it would have been instructive to descend into the realm of the particular to illustrate a general methodological approach by a specific study. George Huaco does this to some extent in his discussion of films, but his macroscopic model (modified Marxian) is too general and his middle-range model tantalizing but summary. Finally, part 6, "History and Theory," is something of a residual category that ranges from Hugh Duncan on Kenneth Burke to considerations of mass society and its effects on art and the artist.

There is then a tentative air about this reader, due partially to the necessarily short or truncated studies, due as well to the number and variety of topics covered. There are, as there must be, areas left unexplored—nothing, for example, on the influence of art on society, a lack which the editors justify (p. xiii) by the absence of sound studies on the subject. Still, and especially given the exploratory nature of this volume,

something might have been included (on mass communication, studies of propaganda, the mass media, even something by McLuhan). Nor is there anything on content analyses, which are perhaps more typical of what sociologists have done to date (although most deal with literature of doubtful aesthetic quality). In many respects, the lacunae of this reader are those of the sociology of art itself, which is unintegrated at the least and unsystematic at best. Albrecht's introductory essay reviews general classifications of art and its functions in the theoretical schemes of Parsons and others. Relating these to specific studies was beyond his scope and intentions.

The view from France is different indeed. Whereas many of the contributors to *The Sociology of Art and Literature* would be perplexed at the appellation "sociologist of literature" (art), the French practitioners clamor for the title, and this despite the training of most of them in literature. Whatever their provenance or institutional affiliation, those who call themselves sociologists of literature in France are conscious of undertaking *sociological* studies. Despite the lack of agreement as to what this entails and regardless of one's opinion of the divers definitions promulgated, there is no doubt that the work of the French is informed by a "discipline consciousness" missing from the dispersed American endeavors (I say American since, the foreign scholars represented in the Albrecht et al. volume notwithstanding, the perspective has been set by American sociologists).

Le littéraire et le social reflects this more focused orientation. It was produced under the direction of Robert Escarpit, professor of comparative literature at the University of Bordeaux, who, by dint of dogged persistence over many years, has created an institutional framework for researchers of all persuasions, from France and elsewhere, in the Institut de littérature et de techniques artistiques de masse. Escarpit's work over the past 15 years or so has mainly consisted of empirical studies of reading publics, patterns of distribution and consumption, the determinants of commercial success.

The article here entitled "Succès et survie littéraires" touches on problems such as the age of optimum productivity, the ever-increasing importance of book reviews, the writer's similar status in socialist and capitalist systems of government, the nonliterary functions of books. This and his other two articles in this collection (and the one on publishing translated in *The Sociology of Art and Literature*) are generally representative of Escarpit's concerns and the careful, empirical approach he has followed. (See also the appendix on the reading habits of army recruits.) In this he is closer to some of the work of American sociologists, especially in mass communication, than his younger collaborators represented in *Le littéraire et le social*. The considerable data collected tend to lack a unifying perspective, and despite the importance of the problems raised (in particular the article "Littérature et développement" on the cultural imbalance between the developed and the "underdeveloped" countries), one is almost

tempted to ask, data for what? Indeed, G. Mury all but does so (pp. 205–6). Data on who reads whom and what, where, and when needs to be ordered by hypotheses about *why*.

At the opposite end of the spectrum lie the theoreticians for whom in the beginning there was Marx, who begat Georg Lukács, who begat the late Lucien Goldmann (represented in the Albrecht et al. volume). For Goldmann and his disciples, the sociology of literature views the social class origin of a literary work as its genesis. Goldmann exercised considerable influence—almost every contributor to *Le littéraire et le social* felt it necessary to tackle him, for good or for ill.

The articles in *Le littéraire et le social* fall into three categories. First, the review articles of Escarpit, J. Dubois, and P. Orecchioni discuss past approaches, from Goldmann's sociology of creation to the content analyses of certain American sociologists, from qualitative to quantitative analysis, and the functions of each. All three stress literature as a communication process rather than as a product and imply that this is where the future of the sociology of literature lies, not in the Marxist-oriented reductionism of Goldmann. So, too, C. Boazis criticizes Goldmann for failing squarely to confront the issue of causality between literary structure and social structure and calls for research on the mechanisms of interaction between the two in order to go beyond simple parallelism of structures (p. 112). He is less than clear on just how this is to be accomplished, but like G. Mury's exhortation to Marxists to engage in empirical studies to support their theories (p. 220), this perspective at least points research in the right direction.

Like Boazis, H. Zalamansky focuses on the work of literature itself, but unlike him he proposes to study its ideological content as an indicator of the solutions, ideas, problems, and models proposed to the reading public He, too, criticizes the excessive generality of Goldmann and others who concentrate exclusively on works of recognized aesthetic merit, which may but probably are not representative of a given period in time. The third groups of articles centers on the public. N. Robine considers problems of motivation: why people read and what conditions historically favored the development of an extensive reading public. R. Estivals's discussion of "bibliologie" and "biliométrie" treats quantitative indicators of intellectual and cultural production and the potential contribution to the study of innovation, evolution of the market, and changes in intellectual activity over time.

Le littéraire et le social was obviously conceived in a very different spirit from *The Sociology of Art and Literature*. More than an assessment and sampler of past work, it presents working papers for the future. It is more limited, if only because it treats literature alone. More closely tied to French research, the Escarpit volume also presupposes (and essentially requires) a certain familiarity with the debates current in French intellectual circles over structuralism, semantics, linguistics, formalism, Marxism (and an ability to decipher the jargon, especially in Boazis's article).

Furthermore, whatever their own intellectual persuasions, American sociologists will find the French largely ignorant of work they consider significant: of Mannheim in the sociology of knowledge; of the theories of culture of Sorokin, Parsons, Kroeber; the analyses of ideology of Clifford Geertz, and others. The programmatic nature and the inevitable repetitions lend these articles an air of superficiality, although covering the surface —raising problems not solving them—is obviously the goal.

Yet in other respects these works are not so dissimilar. In both each writer tends to start anew, choosing his filiation and affiliation, indeed defining his own brand of sociology of literature, while he neglects or remains ignorant of others not congruent with this project. If it is ever to come into its own, the sociology of art and literature needs improved communication and, ideally, work that will incorporate the best of the traditions represented here. Then perhaps we can avoid the fallacy of misplaced concreteness inherent in unordered data or descriptive material no less than the excesses of unsubstantiated generalities. If these works stimulate interest in the sociology of literature, we may hope that they will also engender new perspectives.

The Cloak of Competence: Stigma in the Lives of the Mentally Retarded. By Robert B. Edgerton. Berkeley: University of California Press, 1971. Pp. xv+233. $2.65.

Walter R. Gove

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This is an ethnographic study of 48 mental retardates living in the community. The subjects were a convenience sample (i.e., geographic residence) from a cohort of 110 mental retardates who were the most competent socially and most able intellectually of the patients who were hospitalized at Pacific State Hospital over a 10-year period. Because of the stringent selection, these subjects were much more likely than most retardates who have been institutionalized to make an adequate adjustment in the community.

The book is well written and intrinsically interesting, although the two sections where the case histories of the subjects are presented are tedious. Edgerton starts with a brief introduction to the problem of retardation and the methods used in the study. He then has a very long chapter (81 pages) devoted to detailed portraits of four members of his sample. The third chapter deals with some of the central concerns of the retardates, namely (1) making a living, (2) sex, marriage, and reproduction; and (3) leisure activities. The fourth chapter deals with the subjects' endeavor to seem

normal to others and themselves. The fifth chapter focuses on the role of "benefactors": one of the most important findings of the study is that even among this relatively competent group of retardates the subjects almost invariably required the assistance of others. These benefactors assisted in the management of daily affairs and/or crises (which the retardates frequently precipitate) and helped the subjects pass as normal. The final chapter summarizes the findings and draws a few straightforward implications for institutional programs.

As with most ethnographic accounts the book is largely atheoretical; however, theoreticians will find it suggestive and heuristic. One area of particular interest is that of self-identity. As Edgerton points out, these former patients are not social deviants who have rejected the normative expectations of the "outside" normal world. Instead their every effort is directed toward effecting a legitimate entry into this world. Because the subjects are so open and their life so relatively simple, the basic components of a positive self-identity among these subjects appear more sharply than they do in normals. The role of a job, of marriage, of children (most of the subjects had been sterilized), of material possessions, and of the many skills we take for granted (e.g., the ability to read, to make change, to keep time, to converse) in maintaining a normal self-identity become particularly clear. It seems to me that the understanding produced by this book of the problems mental retardates have in maintaining a normal self-identity will assist in the understanding of the problems and behavior of other disadvantaged persons.

As someone concerned with societal reaction theory, I was particularly interested in the implications of this study for that perspective. The ex-patients uniformly saw their hospitalization as stigmatizing and attempted to hide their past from others, as the societal reaction perspective would suggest. However, as Edgerton makes clear, hospitalization, in a number of ways, helped to enhance and even aggrandize the self-image of these retardates. Furthermore, upon returning to the community, their past hospitalization served an important function in maintaining a modicum of self-esteem. Briefly, the subjects attributed the problems they had in the community to their experiences in the hospital (they uniformly insisted they were inappropriately hospitalized), and this allowed them to deny that these problems were due to retardation. Societal reaction theory also suggests that having been institutionalized would cause others to treat the subjects as deviants. However, the retardates' benefactors, who almost uniformly knew of their retardation and usually of their hospitalization, were generally involved in a "benevolent conspiracy" which consisted of helping the subjects deny their retardation. The question of societal reaction, stigma, and self-esteem appear, from this study, to be much more complex than the societal-reaction theorists have led us to suspect.

In conclusion, this book will be useful to students of self-identity and should be required reading for persons interested in the concerns and behavior of mental retardates.

The Lively Commerce: Prostitution in the United States. By Charles Winick and Paul M. Kinsie. Chicago: Quadrangle Books, 1971. Pp. ix+320. $8.95.

James L. Wunsch

University of Chicago

Prostitution, the ancient social evil, in New York City at least, is still controversial. Mayor Lindsay ordered the roundup of streetwalkers in the Times Square area; a self-righteous judge denied bail to one of the alleged prostitutes, and women's liberation protested: why do the pimps and "johns" escape punishment while these unfortunate women are harassed? There is even now talk of legalizing the trade.

Charles Winick and Paul M. Kinsie understandably believe that the problem is too often treated irrationally and without sufficient information. Their new book, a study of prostitution in the United States during the last half century, held promise of answering the need. Mr. Kinsie and Professor Winick, respectively former investigator and former research director of the American Social Health Association, had access to the files of an organization which has been studying the problem for more than five decades. Two thousand interviews were conducted across the country over the past 10 years. The U.S. Department of Justice and the Russell Sage Foundation supplied funds for additional research and typing.

The subject is considered from several point of view, including that of the prostitute and her client, policeman and judge, pimp and madam, even cabdriver and bellhop. Winick and Kinsie suggest that since 1920 overt prostitution has declined. Where the brothel and red-light district were once commonplace, now even the experienced cabdriver cannot find "the action." Streetwalking may have become more concentrated in parts of certain cities (white men will no longer travel to black neighborhoods for women), but this too has declined. The prostitutes of the infamous whorehouse have been replaced by the call girl living in independent isolation in her highrise apartment.

The decline of bold and open prostitution in a large number of cities is attributed to "vigorous law enforcement" (p. 225). Effective measures to suppress the business were taken during the First and Second World Wars when federal officials determined that troops sent to fight overseas should be free from veneral infection. There was also a marked decline in prostitution during the 1950s, while over the past 20 years legislation has become increasingly stringent.

The authors might have given greater stress to factors other than law enforcement to account for this change. Improved methods of birth control and changing moral standards enabling respectable women to have premarital intercourse and more satisfactory relations during marriage may have sharply cut into the demand for prostitutes. John Gagnon ("Prostitution," in *The International Encyclopedia of Social Science* [New York:

Macmillan Co., 1968], p. 593) argues that these and other factors, far more than police action, will explain at least the decline in incidence of contact between prostitute and client.

That law enforcement did have substantial effect on prostitution is a thesis which at least deserves respectful attention. The problem with this book, however, is its complete failure to document and consider facts systematically. The reader is assaulted with bits and pieces of information from dozens of cities, but these are not organized to make effective argument. For example, the closing of red-light districts during World War II, notably in Terre Haute, Indiana, and Hawaii, is allegedly related to the fall at the same time of venereal disease and crime rates. The conclusion is that "when prostitution declines, so do other crimes" (p. 225). But the matter is not pursued in the postwar period where the relationship between the decline of prostitution, VD, and the crime rate is not self-evident. Significantly, it is mentioned that prostitutes, many of whom are addicts, attract to an area the pushers and other addicts who commit crime. But what indeed happens to the prostitute and these others when the red-light district is "cleaned up"? The matter is not discussed. Typically, questions here are considered neither extensively enough to generalize nor intensively enough to illuminate the mechanism of change.

An act of faith is demanded to appreciate the use of statistics. The authors allege that the 95,550 arrests for prostitution and related offenses in 1968, "a typical year" (p. 4), represents the number of full-time working prostitutes. The unwarranted assumption is that the multiple arrests for individuals is equivalent to the number of those who escape arrest. Winick and Kinsie consider their figure to be conservative because some students, unnamed or acknowledged, have offered even higher estimates. Of the many undocumented facts, the determination of the following statistics is left unspecified: "The average full-time prostitute works six days a week and has three clients daily" (p. 4); "many streetwalkers often average twelve hours a day" (p. 26); "the average call girl sees perhaps sixty men a month" (p. 176).

Some sources which are cited further undermine the book's authority. Reference is made to Nell Kimball, "one of the best known madams" (p. 106). In selecting quotations from her recently published memoirs (*Nell Kimball: Her Life as an American Madam*, ed. Stephen Longstreet [New York: Macmillan Co., 1970]), the authors might have detected that portions of her account were paraphrased directly from the works of a popular historian, Herbert Asbury, who died several years ago. It is doubtful whether Nell Kimball ever existed. Her memoir is fake. (A full explanation of this fraud may be found in *Journal of Social History* [June 1972].)

Finally, when we are told that Clarissa Harlowe "works in a brothel" (p. 17), we realize that the authors have neither read Richardson's novel (*Clarissa*) nor even a good plot outline.

Of 2,000 interviews ("actual names of specific respondents and other persons are given wherever possible" [p. vi]), three citations may be found

in the footnotes; there are two references to unpublished material in the files of the American Social Health Association.

In *Prostitution and Morality* by Harry Benjamin and R. E. L. Masters (New York: Julian Press, 1964), a book with a chapter format strikingly similar to *The Lively Commerce*, an entirely different point of view is maintained. Where Winick and Kinsie argue that the brothel and red-light district contributed to high rates of crime and venereal disease, Benjamin and Masters question any such relationship and assert that brothels sanctioned and properly regulated by the state might provide a reasonable service to the community. In the course of their argument, Benjamin and Masters assail the American Social Health Association for "having distorted some facts about prostitution and suppressed others . . . this organization has done more than any other . . . to thwart a sane and scientific approach to the problem in the United States" (p. 341). Benjamin and Masters were unconvincing in proving this, but now Winick and Kinsie with their efforts begin to make the charge seem credible.

On Record: Files and Dossiers in American Life. Edited by Stanton Wheeler. New York: Russell Sage Foundation, 1970. Pp. 25+449. $12.50.

Ernest van den Haag

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As population density increases so do transactions among people who do not know each other. This creates a mounting demand for accurate records of career and personal data. While contributing to this demand modern technology has also made it possible to supply it, because extensive records can be kept and fairly easily retrieved at low cost now.

In the volume under review expert contributors describe, in almost painful detail, how records are kept. These descriptions will be invaluable to anyone wishing to do further work on the subject. Such work is needed. For while records answer questions, they also raise questions about the legitimacy of questions and answers. What should be recorded? Who should have access to records? Who is allowed, or obligated, to make what information available to whom? What about misinformation and correct, but irrelevantly damaging information? What is in the public domain, and what is private? What about interpretation, misinterpretation, and misuse?

The authors of *On Record* valiantly attempt to answer such questions. Success is necessarily spotty. But they have the great merit of throwing the theoretical problems into relief by describing the actual processes of record keeping and by sketching the distribution of the authority which shapes these processes in numerous institutions.

Collecting compelling information, keeping records, and making them available has become quite controversial as damage from misinformation has become patent and frequent. People are jealous of their threatened privacy. We have become afraid of misinformation, or misused information

which can hinder an individual's legitimate future activities when his recorded actual or alleged past leads people to deny him opportunities.

Some opposition to data collecting is legitimate, but some is misdirected. I can see no justification for compelling people to give census data (utility hardly justifies compulsion). But there are objections also to allowing the government to collect dossiers, by means other than compulsion, on citizens, subversive and otherwise. The fears underlying these objections are understandable and sometimes even justifiable. Yet total opposition seems besides the point. Collecting (though not compelling) information is everyone's right, and some people's duty. Police departments must have information on suspects; business needs information on consumers, employees, and credit risks; government needs information on citizens. Those who try to prevent the keeping of records aim too broadly. However, everyone has an interest in preventing misinformation from being recorded and distributed, or information from being misused.

How can this interest be protected? The law might impose fairly strict liabilities on distributors of information; liabilities which cannot be discharged by disclaimers. Liability should be canceled only upon giving the subject of the information a reasonable opportunity to comment and either using the comment to his satisfaction or adding it to the information. Such a rule might be applied to all information regularly disseminated to third persons or within the government and other institutions when used to decide on promotion, hiring, training welfare matters, etc.

Information can never be harmful; only misuse and misinformation can Hence efforts should be made toward a framework which minimizes only these. Anyone who wishes to try his hand in elaborating appropriate rules and institutions to safeguard against abuse of records will find in this book not only the materials he must work with, but also many useful hints toward this end.

They're Bringing It All Back: Police on the Homefront. Edited by National Action and Research on the Military-Industrial Complex. Philadelphia: American Friends Service Committee, 1971. Pp. ii+133. $1.35 (paper).

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In the last few years, a number of radical research groups have helped us to understand better the distribution and exercise of power in America by revealing hidden relationships among corporations, universities, and the government. *They're Bringing It All Back: Police on the Homefront* is a collection of such revelations, focusing on rapidly developing ties between urban police departments, universities and research centers, a number of large corporations, federal law enforcement agencies, and the military. Results of this unholy alliance include the development of exotic weapons for crowd control and the transplantation of weapons and combat tech-

niques used in Vietnam to the homefront for domestic counterinsurgency; a major step-up in domestic intelligence operations, especially but not exclusively directed against black and student groups; "professionalization" of police departments and increased coordination of their efforts with the Justice Department, especially through federal funding or direction of projects carried out by local police, ranging from purchase of new equipment to the arrest of nearly 900 demonstrating black college students in Itta Benna, Mississippi; a substantial increase in federally financed research on crime causation and control and on ways to increase the efficiency of the criminal justice system; and training programs for police departments of American allies, as part of a worldwide Vietnamization program. The documentation is thorough and includes lists of government research grants, training programs for foreign police, and a list of colleges training local police forces. A supplement reprints documents stolen from FBI files in Media, Pennsylvania.

The documentation provided in this volume will be useful in combating the myth often used to defend research of this kind—that it is a neutral, value-free enterprise motivated mainly by the desire to better understand society or to solve social problems, help the poor, etc. Overwhelmingly, the projects funded are "applied" and reflect the needs of those who manage the system, not those it processes. Such themes as how to control riots; detect drug users; catch criminals, process them more efficiently, and control them while in custody, predominate. Only one of the 100 grants awarded in 1970 by the National Institute of Law Enforcement concerned the decriminalization of harmless activity; only one concerned business crime; no grants were awarded on ways to eliminate racial or class discrimination on the part of police, judges or prison officials; none on how to protect or expand civil liberties; none on how to change the social environment thought to be conducive to crime. The essays also answer the other myth—that it is good for the universities to train police because this will liberalize them.

An informative study like this inevitably raises questions beyond the study itself. What should be done? How to do it? Can the changes needed to make the police more responsive to human needs be made short of a social revolution? If not—and this is the inference of a number of the essays—how can that be brought about? There are of course no easy answers to these questions, but the information contained in the volume under review will prove helpful in thinking about them.

The Gift Relationship: From Human Blood to Social Policy. By Richard M. Titmuss. New York: Random House, 1971. Pp. 339. $6.95.

A. H. Halsey

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No American, as Nathan Glazer has said, can read this book without shame. This is not merely because of what everyone knows, that American

health services are expensive, unequally distributed in relation to need, imperfectly coordinated, and ineffective in terms of health standards. Nor is it only because they are inefficient (about 30% of all blood collected in the United States is wasted through technical or administrative inefficiency compared with 2% in England). It is because, and this is the essential burden of Richard Titmuss's attack, they reflect the structure of a society which is inhumane.

The argument, in direct descent from, and easily the most dramatic postwar expression of, R. H. Tawney's socialist critique of capitalist society, is focused on social arrangements for linking supply to demand in the case of blood. From his study Titmuss concludes "that the commercialization of blood and donor relationships represses the expression of altruism, erodes the sense of community, lowers scientific standards, limits both personal and professional freedoms, sanctions the making of profits in hospitals and clinical laboratories, legalizes hostility between doctor and patient, subjects critical areas of medicine to the laws of the marketplace, places immense social costs on those least able to bear them —the poor, the sick and the inept—increases the danger of unethical behaviour in various sectors of medical science and practice, and results in situations in which proportionately more blood is supplied by the poor, the unskilled, the unemployed, Negroes and other low income groups and categories of exploited human populations of high blood yielders" (p. 245)

These are grave charges. Some sociologists may want to say that as sociologists they are not professionally interested in moralizing. They may expect instead that a review in the *AJS* would address itself to the merits, if any, of the nonevaluative theory and the validity of the empirical propositions which are advanced. For them, let it be said that Titmuss's survey of the attitudes and motives of blood donors in Britain is of high competence, and that the assembly of relevant data from the voluminous scattering of unevenly accurate statistics in the United States and other countries all over the world have been undertaken with minute care and caution. At this level there can be no quarrel with the standards of a superior craftsman.

At the level of sociological theory Titmuss is perhaps more vulnerable Three criticisms seem to be possible. The first concerns the distinction which Titmuss draws from Marcel Mauss's *Essai sur le Don* (1924) between "economic exchanges" and "non-economic exchanges" where the former are part of self-interested trading and the latter symbolize the social bond between the participants. Not all modern anthropologists would accept this functional distinction. It is, however, crucial to Titmuss's analysis of the gift in advanced industrial societies, which he sees as a mode of integration between individuals and society and all the more altruistic because of the more remote and anonymous connection between giver and receiver in such societies. But whether or not Titmuss interprets Mauss, Lévi-Strauss, and Malinowski correctly concerning donor relationships in primitive societies is a relatively unimportant question compared with his own theory of a kind of Gresham's law of selfishness such that the

institution of the market undermines the social integration of individuals through the erosion of opportunities for the expression of altruism.

The second criticism, which is directly relevant to this last proposition, has been put forward by Nathan Glazer (in *Public Interest*, no. 24 [Summer 1971], pp. 86–94) as a criticism of Titmuss's use of the word "freedom." Glazer questions whether the disincentive to give freely actually operates in fact. He bases his doubt on a casual inquiry among Harvard students who had responded to an appeal for blood donations. It turned out that the fact that others might subsequently make a profit from processing and selling blood was not a consideration which was ever raised among the donors. He argues further that, though the rising proportion of bought blood in the United States might be held to demonstrate a decay in altruism through commercialization, it would still be questionable whether a restriction of freedom was involved. Glazer's point is well taken. Titmuss may have overdone the paradoxical rhetoric of a socialist attack on the freedom of the free market.

The third criticism is fundamental. It is that blood giving and blood markets may not serve adequately as comprehensive indices of all the social determinants of altruism and egotism in a society and may even be misleading. Titmuss has proceeded "from human blood to social policy" on the assumption that blood is the critical case. It is true that blood has traditionally magical overtones; but are these so strong in a modern scientific and secular society as they once were? And in any case are there not more critical tests, for example the proportion of GNP which is allocated to the social services or the degree of inequality of income distribution? Is the British and American comparison so stark on these alternative measures?

Nevertheless, though blood and the private market may not encompass the whole debate, we are still left with a magnificent study of the character of social integration in modern society which may be expected to stay at the center of discussion for years to come.

The Chippewas and Their Neighbors: A Study in Ethnohistory. By Harold Hickerson. New York: Holt, Rinehart & Winston, 1970. Pp. x+133. $2.65 (paper).

Nancy Oestreich Lurie

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Harold Hickerson demonstrates very effectively that documentary sources handled with rigorous methods of history and ethnology together can elucidate understanding of given topics and allow for the formulation and testing of theoretical propositions far beyond the reach of conventional historiography or ethnographic field research taken alone. This, in effect, is the definition of ethnohistory, Hickerson's basic concern, exemplified in

this book by Chippewa data. Hickerson provides critical documentation for particular problems that have broad theoretical significance in exploring certain aspects of cultural dynamics in general.

Opening with discussions of the nature and methods of ethnohistory, the Chippewa themselves, and historical approaches in cultural anthropology as a whole, Hickerson makes clear that it is not his objective to write a general ethnology or even ethnohistory of the Chippewa. Rather, he has chosen three, fairly discrete, problems regarding culture change among the Chippewa which could not be studied at firsthand in the field at this late date or even explored effectively through collection of oral tradition still available among the Chippewa (which, in any event, Hickerson tends to view with perhaps excessively disdainful suspicion).

The first case involves the assemblages of people designated "nations" in the earliest 17th-century accounts from the western Great Lakes. These groups are usually assumed by later writers to be no more than archaically named versions of the now familiar Chippewa "tribes" and "bands" of the fur trade, treaty cession, and reservation periods. Hickerson makes a convincing case, however, that the first chroniclers actually caught sight of precontact social organization of the Chippewa. His careful review of early nomenclature and other bits of data suggest the presence of single clan settlements with contiguous territories, "confederated" by common language, customs, and affinal kinship links based on clan exogamy. As a doctrinaire cultural evolutionist, Hickerson believes further study, especially in linguistics, will prove these were originally matrilineal clans, but preserves his always commendable impartial adherence to documented fact in discussing the Chippewa for what they were in historic times, that is, patrilineal. When the large clan villages broke up into smaller, more mobile, and territorially expanding bands in response to the demands of the fur trade, they split not along clan lines but across them as groups of families, each representing several to all of the original clans. Thus, they preserved common Chippewa identity and, it should be added, localized pools of eligible marriage partners from different clans despite the wide distribution of the relatively small Chippewa population over an area that ultimately stretched from Lake Huron to the northern plains.

Hickerson's second case concerns the Medicine Lodge or Midewiwin, a society of religious initiates that figures prominently in early, but not the earliest, accounts and is still a going concern among some of the more conservative Chippewa communities and other woodland tribes. Because of its ubiquity, the Midewiwin is widely assumed to be a survival from aboriginal times. Not so, says Hickerson, echoing and documenting more convincingly a generally neglected view first expressed by Paul Radin regarding his study of the Winnebago Medicine Lodge that it was induced by the effects of the fur trade (Paul Radin, *The Road of Life and Death* [New York: Alfred A. Knopf, Inc., 1945], pp. 50–51, 74). While the trade brought material enrichment of which the Chippewa availed themselves eagerly, it also resulted in profound repercussions in the aboriginal culture. The Medicine Lodge may thus have been the first and most successful of

a series of postcontact revitalization or nativistic movements to sweep the Great Lakes Indian peoples in times of crisis, providing supernatural rationale to reintegrate native culture and maintain sociocultural continuity in the face of irresistible or unavoidable innovations having disorganizing and destructive effects.

Finally, Hickerson expands upon a problem he has dealt with in earlier studies to which he refers, that the presumably ancient animosity between the Dakota and Chippewa was, in fact, recent and the result of the fur trade. In a concluding chapter to this argument, "Biome and Warfare . . . ," Hickerson draws skillfully on data from the natural sciences regarding floral and faunal successions in historic times in the western Great Lakes region to demonstrate ecologically that there was utility in perennial warfare for both the Dakota and Chippewa. That the Indians understood such biosocial ramifications is clear from reports of their own statements made in resistance to white efforts to induce them to make peace and get down to the important business of trapping an area that could not be exploited for the profit of the fur traders because of the Indians' hostilities. This "debatable zone" ran in a northwesterly direction from the Chippewa River in Wisconsin through the headwaters of the Mississippi in Minnesota to the Red and Big Sioux Rivers. Unlike large sections of the areas occupied by the Chippewa and Dakota, this zone was ideally suited to the habits of the whitetail deer on which both groups of Indians depended heavily for food. By keeping the area constantly and mutually dangerous, the Indians discouraged traders (whose dealings were generally confined to one tribe or the other) from establishing posts that would attract Indians to settle in the vicinity and thus quickly deplete and drive off the supply of large game. "Where there was peace there was starvation. But ironically, success in war must also have meant starvation, for that would have involved Dakota or Chippewa, whichever was successful, overrunning the country at the expense of the other, hence having free use of the once-contested area, and, because of the resulting excessive demands on game, namely deer, exploiting it to depletion. There was, then, a *selective advantage in not winning,* but only through steady attrition maintaining a precarious balance through which the neutral zone could be maintained as a deer reserve to be tapped vicariously on either side, but not exhausted. . . . Better a steady little than nothing" (pp. 117–18).

All in all, Hickerson's book is a solid contribution to the anthropological literature in regard to gaining new value from old data, and as a highly useful teaching manual of ethnohistorical theory and method. We may hope that his example is an inspiration for other scholars, particularly younger people seeking untapped areas for research, to go and do likewise. In addition to the exciting sense of sleuthing that the often denigrated "library research" offers anthropologists, it affords moments of literary delight when one stumbles upon, for example, the ineffably original spelling of a Peter Pond or the pragmatic logic of a Little Crow who admits that war, in principle, is bad, but the occasional loss of a man on either side every year is better than wholesale starvation for all.

Frontier Folkways. By James G. Leyburn. New Haven, Conn.: Shoestring Press, 1971. Pp. x+291. $9.25.

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Frontier Folkways is an unabridged, unedited version of Leyburn's doctoral dissertation, which was originally released in 1935. It is a study of the growth of new patterns of behavior on frontiers, of the modification of the original customs and institutions that colonial populations brought to this setting. The focus is on changes in the behavior of colonial peoples themselves and not on the effect of the frontier on native populations or the colonial power at home.

Leyburn classifies frontier societies on the basis of their basic exploitative unit and discusses cases relevant to each: small farms (the English in Massachusetts Bay, the French along the St. Lawrence, the English in New Zealand), plantations (the Portuguese in Brazil, English in Australia, and Boers of the Transvaal), exploitative plantations (the Dutch in Java and Spanish in America), and camps (American colonies on the Great Plains and in California). The societies that developed are compared with respect to economic behavior (division of labor, land tenure, standard of living), marital institutions (marriage, divorce, immorality), political behavior (nature of the institutions, legal authority for punishment), social structure (class, rights of women and children), "ethos" or character (temper of political life, foresight, quality of amusement and humor), religion, and demography.

One of the author's goals is to explain the similarities and differences among frontier societies. He closes with a series of "laws," some of which are assumptions and some of which are indeed testable hypotheses. Ecological variables and economic behavior and motives are primary, and the natural environment looms somewhat larger than contact with indigenous populations in explaining the development of new institutions. More generally, Leyburn employs a kind of synthetic evolutionary model of frontiers. Colonial populations are usually an atypical subset of the society from which they came, and the pace of diffusion and invention are accelerated by the frontier situation creating additional variety. New selective pressures determine that those few innovations that survive will give a new character to the society.

Beyond these touchstones, Leyburn's theorizing ranges from the synthetic to the eclectic. He allows that both conscious and unconscious responses to the frontier have had important effects on social evolution Much of the best that evolutionary and acculturational approaches have to offer is combined. But he is less successful in dealing with psychological factors. Envy and greed are sometimes used as explanatory variables. And there are casual ventures into the most naïve of culture and personality analyses. Among the French in South Africa, "The vivacity, versatility, and dash which one associates with the French was entirely lost to Dutch

phlegm" (p. 106). Leyburn's view of the role of the social scientist is similarly out of date: "A social scientist never feels called upon to ask of an institution, Is it merciful and just? He asks rather, Did it work? or How did it work?" (p. 178).

That such problems should exist in a work written 35 years ago is understandable. That neither Leyburn nor social scientists as a group have solved the major ones is not. On a theoretical level, the many testable hypotheses about frontiers that Leyburn discusses are largely untested. Analytically, we have the social history and the comparative method. While the former is sociologically interesting, its techniques of verification are analytically unsatisfying. And the evidence that cross-cultural, cross-community, and even cross-sectional studies do not produce sequences that can be equated with actual patterns of temporal variation grows by the year.

The frontier literature, as Leyburn's work demonstrates, is a rich source of data on demographic, economic, organizational, and, sometimes, attitudinal change. Not infrequently, quantified or quantifiable data can be obtained. Neither Leyburn's problems nor those of today stem from the nature of the data. Rather, they stem from our approaches to them. At one point, the author observes that the variety in frontier situations represented in the book could have been replicated in a detailed consideration of one historical sequence, that in America. But the preference for spatial over temporal variation and comparison triumphs. And still today, diachronic data, when available, are forced into synchronic molds. Trend and other dynamic analyses are largely unused. Our ability to understand and explain patterns of temporal variation will continue to remain largely unrealized until we accept a fundamental imperative: it is necessary to observe and measure processes of change in order to understand and explain changes.

The Sociology of Occupations. By Elliott A. Krause. Boston: Little, Brown & Co., 1971. Pp. xvi+398. $7.95.

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The Sociology of Occupations is a substantial contribution to a subfield of sociology that needs a great deal of improvement to become theoretically sophisticated, of use to anyone outside of government, business, and the union bureaucracy, and just plain interesting. Elliott Krause's writing has none of the turgid quality that marks the work of so many sociologists who go beyond description to analysis. He put sociological concepts to use, making them clarify rather than obscure general relationships among classes of phenomena.

The author takes a critical perspective throughout his book as he describes how occupations' histories, career patterns, practitioners, and functions are shaped by ruling classes and groups as well as by factors internal

to the occupations. His book indicates "the need for a combination of the field of the sociology of occupations and such fields as political sociology and the sociology of knowledge. A political sociology which does not treat the political action of occupational groups is risking irrelevance, whereas an apolitical sociology of occupations and professions misses what has *always* been a major dimension of these groups' actions" (p. 353).

Krause's analysis of occupations is limited because the book is tied together not by theory but by methodology divorced from theory. Nevertheless, his methodology has the virtue of allowing occupations to be examined from several approaches without making the book an eclectic potpourri of observations and insights. The book's first four chapters develop Krause's methodology: the historical approach to the development of occupations; the biographical approach which traces and compares career patterns within and between occupations; the functional analysis of occupations in terms of the societies of which they are a part; and a "conflict of interest" approach. The following 10 chapters apply the methodology to the analysis of specific occupational areas: law, health, religion, the military, business, science, art, education, and public service Krause also applies his framework to activist and "illegal" occupations The summaries provided at the end of each subtopic, topic, and chapter are superb and can profitably be read first to make the sections hang together even better for the reader.

Here is an author in search of Marx. Krause's substantive historical descriptions and summary generalizations could well start to fill the vessels of a sophisticated Marxian theory of American occupational structure, process, and struggle. A more self-consciously Marxian approach might have enabled Krause to overcome the sociological disease of methodological fragmentation. The result might have been a dialectical analysis of the intimate relationships of historical process, group struggle, and personal biography. Nevertheless, the author's feel for a Marxian approach prevents his methodological division of reality from overwhelming him. For instance, in his section on "historical perspective," Krause describes the "deprofessionalization" process during the Jacksonian era as a *struggle* of the working class against the ruling class—this despite his separate section on "conflicts of interest."

Krause's firm but critical command of the sociological literature on occupations and mobility enables him to use that literature to refine, not debunk, Marxian analysis. One result is a much more complex, subtle, and thorough analysis of the contradiction between occupational and class consciousness (pp. 85, 87) than that offered by Dahrendorf in his attempt to debunk Marx in favor of a pluralist model of occupational conflict Another result emerges from Krause's clarification of the contradiction between individual and group mobility (p. 71). This contradiction underlies the legitimating function of those sociologists who measure a group's mobility by measuring individual mobility.

The chapters on individual occupations clearly reveal that Krause's mission is clarification, not debunking or refutation. For instance, he

repeatedly shows in detail how ideology reflects and legitimizes the power of the dominant class. The occupational ideologies of groups that are a part of the ruling class or whose functions resonate with the needs of that class are more likely to be accepted than those of other groups. Krause does not invoke a vulgar Marxian analysis (which is most often a strawman position put forward by an anti-Marxian) to explain this phenomenon, but points out that a group's acceptability and support reflect the group's functional importance to the system being examined. In a capitalist system, groups functionally necessary for the successful operation of the system will be made more acceptable by the ruling class-dominated government (which officially recognizes occupational ideologies by delegating control of training, entrance, and practice to the occupation) and mass media.

Krause's consideration of "occupational altruism" not only points out the relationship between the status, material advantages, and autonomy of an occupation's practitioners and its altruism. It also points out that an occupation's altruism generally ends where challenges to its status, material advantages, and autonomy begin. "Occupational altruism, without a major change in the overall economic-political system, may be more a luxury for the haves than a solution for the have-nots" (pp. 101–2). In many places, Krause notes that co-optation and repression prevent occupational altruism from developing into occupational radicalism. There just "weren't [*sic*] any jobs in the system for those who wanted to be paid for opposing it" (p. 354).

Bureaucracy and Participation: Political Cultures in Four Wisconsin Cities. By Robert R. Alford with the collaboration of Harry M. Scoble. Chicago: Rand McNally & Co., 1969. Pp. xvi+244. $6.50.

Patrick Molloy

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One might ask if four cases constitute an adequate base for a comparative study of urban politics. The merits of *Four Cities* and *The Rulers and the Ruled* could be used in justifying such a strategy. And James Q. Wilson has argued that intensive analysis of a small number of cities enables one to escape many of the measurement problems plaguing more ambitious comparative studies. Present methods of data analysis, in this view, are exploited to proper advantage when the researcher becomes acquainted with the style, ethos, or culture of a few local governments. Robert Alford does not repeat that argument here, although his study is definitely relevant to the continuing discussion. Neither is his work very greatly concerned with the advantages of various numbers of cases.

The Wisconsin cities Alford reports on will nevertheless become familiar to social scientists when this and related reports based upon the same data work their way into several fields of substantive interest. The four cities—Green Bay, Kenosha, Racine, and Madison—ranged in population from

63,000 to 127,000 in 1960. Each used the mayor-council form of government with nonpartisan election by wards. The economic base, to which Alford attributes great weight, differentiates the cities. Green Bay is classified as a traditional trading center; Kenosha and Racine as manufacturing centers; and Madison, the home of the state capital and state university, as a modern professional center. The author provides brief histories of each city, concentrating upon the 1945–62 period, and also devotes attention to such topics as land use and social ecology, business and labor problems, political parties, boards of education, and the community role of businessmen. In some cases, additional topics and events unique to a city are discussed. Although largely descriptive, the chapters on the individual cities are consistent with the central hypothesis developed in the introductory and more theoretical chapters: "The largest and most economically diverse professional center, Madison, should exhibit the highest levels of bureaucratization and participation. The two manufacturing centers, Racine and Kenosha, should fall in the middle" (p. 16).

The first two chapters touch upon numerous topics of interest to sociologists. In broad and direct fashion they stress dominant themes in the study of bureaucratization, professionalization, political participation, and urban government. While the treatment of these matters is very abbreviated, Alford indicates biases or areas of neglect in existing approaches which he proposes to remedy by a more comprehensive consideration of structural and cultural features of communities. Many will appreciate the injection of central sociological concerns into the study of urban politics Occasionally the attempt to attain conceptual precision leads to less than illuminating clarifications such as, "The persistence of relationships, of course, though not absolutely determinative, is closely correlated to a low probability that actors could change the relationship even if they mobilized all of their resources" (p. 4). But these instances are rare and the reader is far more often rewarded by the introduction of notions like structural conduciveness or the treatment of culture advocated by Bendix and Berger Not fully developed and too often merely favorably juxtaposed to other traditions of analysis, their presence nevertheless suggests that the author is attempting to enrich the fare of those accustomed only to learning who governs in yet another set of cities. Alford has elaborated upon these subjects in other articles and, as he explicitly mentions, only a few themes could be covered in the empirical chapters.

Even with the narrowing of questions in later chapters Alford cautions the reader about the indirectness of his data and often reminds him that many questions one would like answered are in fact difficult to answer with the present material. One is left wondering, for example, whether "Madison residents are either really more active or [whether] they feel that being active in a party is 'the thing to do' in their community" (p. 133). But in addition to an extensive survey of leaders and voters, the author draws upon public records, unpublished material, and to some extent personal observations, to support his argument.

Madison consistently reveals higher, and Green Bay lower, rates of bureaucratization (or professionalization) and participation than Kenosha and Racine. In one of the few comparisons in which Madison and Green Bay prove similar Alford points out that the correlation of occupation with party identification is low in both cities. Trade union activity in Racine and Kenosha is probably responsible for the higher correlations found there. In other matters, however, Alford finds it difficult to isolate any distinctive political culture to be attributed to the city itself and not due to the socioeconomic composition of the city. Different types of data would be required to answer the question satisfactorily, but it appears at least plausible to the author that public norms of the cities are an important influence in the process of selective recruitment of leaders.

What Alford labels the traditional features of Green Bay and the modern features of Madison seem to form coherent systems. He is sensitive to change in those systems, and he continually points to the potential role of political leadership in modifying the economic base of a city. Yet there is a static aspect to his analysis. In previous periods there seem to have been no changes in the overall ranking of the cities. Kenosha did adopt a "modern" council-manager plan during the 1920s. But this was finally replaced in 1958 by the mayor-council plan, and almost inevitably so in the view of the author, for he views this plan as more consistent with and appropriate to the local political culture.

The distinction between traditional and modern elements occasionally becomes a troublesome one to maintain. "Clearly none of these cities may be expected to conform to the extreme type of modern or traditional system, precisely because they are all fairly bureaucratized cities within a modern society and polity" (p. 204). But other comparative analysts, such as Samuel Huntington, would argue that the very existence of powerful local governments is quite traditional, and Alford himself recognizes "the historic commitment of the American political system to the principle of local autonomy and the preeminent role of the local electorate or its direct representatives in many policy areas which are removed from popular control in other democratic countries such as Britain" (p. 141). Perhaps it is significant that Daniel Elazar, a political scientist concerned with concrete problems of federalism in his *Cities of the Prairie*, found it desirable to develop a more historically grounded notion of traditional political culture, one which emphasizes the heritage of plantation agrarianism characteristic of the South. The mere differentiation of demand and decision-making units, which Alford takes to be the basis for deciding whether a city is modern or traditional, again appears to be too ahistorical and inflexible a criterion, especially since a wide range of policy issues are not framed with those concerns in mind.

The result is that too often Racine and Kenosha become indistinguishable in the intermediate category, and the main comparison involves Madison and Green Bay. And in the matter of innovative public policy it becomes impossible to decide why Kenosha and Racine are not more

innovative than Green Bay when the former two cities by most measures must be considered more modern. What promises to be a study of four cities frequently becomes a study of two or three cities.

Alford's observations about the shortcomings of existing approaches, his adept presentation of a diversified set of data on leader and mass orientations, and his effort to separate analytical elements of urban politics too often confused makes this an important source for students in a number of fields. The author advises the reader when evidence is too meager to support a proposition adequately, and he is straightforward about the merits and defects of alternative methodological strategies. But for those interested in effective community action the constraints and resources provided by local political cultures will remain barely understood until more adequate notions replace the rather abstract versions of tradition and modernity. The systematic analysis of larger numbers of medium-sized cities will also gain in theoretical strength when the concept of modernity is less closely identified with bureaucratization and participation.

Automation and Alienation: A Study of Office and Factory Workers. By Jon M. Shepard. Cambridge, Mass.: M.I.T. Press, 1971. Pp. 163. $7.95.

Automation and Behavior: A Social Psychological Study. By John Chadwick-Jones. New York: John Wiley & Sons, 1970. Pp. xi+168. $8.95.

Ely Chinoy

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Both of these books deal with the same general problem, the impact of technological change upon workers, but they differ markedly in their focus, their methods, and the kinds of conclusions at which they arrive. Neither, unfortunately, adds much to our knowledge.

Jon Shepard's study tries to test a general hypothesis about the relationship between technology and the level of alienation to be found among workers in both industry and the office. He begins with the assumption that "there is a curvilinear relationship between the phases in the man-machine relationship and the degree of differentiation in the division of labor" (p. 9). With greater mechanization the division of labor increases, but with the introduction of automation the division of labor diminishes. Since alienation is presumably related to the division of labor, there should then also be a "curvilinear relationship" between the development of technology and the level of alienation.

The data used to test the hypothesis are drawn from interviews with three groups of manual workers and from questionnaires based on the interview schedule administered to several groups of white-collar workers. The three groups of manual workers were craftsmen (nonmechanized) and assembly-line workers (mechanized) in an automobile plant and operators in an automated oil refinery. The white-collar employees were clerks (non-

mechanized), office-machine operators such as typists or key-punchers (mechanized), and computer programmers, operators, and systems analysts (automated) in several insurance companies and a bank.

Alienation was defined, following Melvin Seeman, in terms of powerlessness, meaninglessness, normlessness, an instrumental work orientation, and a low level of personal involvement in one's work. Scales for each of these dimensions of alienation were then constructed and the various groups compared with one another with respect to the proportion of each whose alienation scores were above the median score for the whole sample.

The findings consistently support the hypothesis that the study was designed to test. On each of the dimensions of alienation the proportion of respondents whose scores were above the median was lower, with few exceptions, for both the nonmechanized workers (craftsmen and office clerks) and for those monitoring or operating automated technology in factory and office than for mechanized blue-collar and white-collar workers.

In confining his analysis to the test of one hypothesis, Shepard ignores important questions suggested by his own data. Why, for example, do only 19% of the craftsmen rank above the median with respect to powerlessness while almost half rank above the median on the other dimensions of alienation? Why, in fact, do almost half of the craftsmen score high on four of the five alienation variables? Similarly, why do almost half of the nonmechanized white-collar workers score above the median on normlessness and on lack of self-evaluative involvement and more than half above the median on powerlessness, meaninglessness, and instrumental work orientation? Such data suggest that however important technology may be in affecting workers' responses to their work, other variables may be equally or even more important sources of alienation.

One cannot, of course, fault a researcher for not doing what he did not set out to do, unless what is left out might affect the results. As Shepard himself notes, "factors affecting attitudes toward work are too many and their interrelationships too complex, to assume that the technologically determined attributes of the job alone constitute an explanatory variable" (p. 126). Yet, except for sex and size of organization, the latter dealt with very briefly, there is virtually no reference in the book to other variables, some of which might not only have an independent impact on work experience but might also affect how workers respond to technology itself. Among these variables are basic institutional and cultural factors whose neglect may stem from the fact that the concept of alienation, which originally referred to a relationship between man and his society and culture, is reduced, following much current practice, to a set of social psychological categories.

Automation and Behavior deals with the impact of automation upon a group of steelworkers. It is concerned with such detailed questions as individual adjustments to changes in the job, levels of satisfaction and dissatisfaction, social relationships on the job, the role of supervision and responses to it, and changes in management. The data are based upon observations made in 1956–57, before automation was introduced, and in

1962–63, after the new technology was in operation, with follow-up interviews that continued until 1966.

There is little in the research that adds or elaborates upon the findings reported in two earlier studies of automation in the steel industry—Charles R. Walker's *Toward the Automatic Factory,* and *Technical Change and Industrial Relations,* by William Scott and his collaborators. Chadwick-Jones's general conclusions, that workers should be prepared for change, that job enlargement would increase workers' interest in their jobs (a conclusion Shepard also arrives at), that social behavior on the job be taken into account in "forward planning," are hardly new, and the detailed data are given in such a pedestrian fashion that one can only plod doggedly through the book looking for the all-too-few nuggets.

Social Fragmentation and Political Hostility: An Austrian Case Study By G. Bingham Powell, Jr. Stanford, Calif.: Stanford University Press, 1970. Pp. 201. $7.50.

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This is a case study of political life in a small industrial Austrian city. In early 1967, the author carried out a sample survey of 200-plus citizens in Hallein. From his base in nearby Salzburg both before and after his survey, he visited the city, talked with party and municipal officials, studied budgetary data, and browsed through newspaper files. From his survey, data were obtained about Hallein citizens: their partisan sentiments, issue orientations, group memberships, social-cleavage positions, and the sense of political hostility and distrust manifested by SPO and OVP adherents toward one another—the so-called *lagerdenken* mentality which others have seen as pronounced in postwar Austria. Much of his text seeks with these data to clarify how "social fragmentation" and "political hostility" affect one another, not only in the way citizens view their community but also in the performance of local political elites. A rather useful analysis of the municipal budget is made, highlighting the ways in which structural constraints affect the locality's "system capabilities." Patterns of city-council and party decision making are described, as exemplified in a small number of political struggles that arose in the 1960s, but his examination of elite cleavage lines does not rest upon any hard evidence. The study is not rich in contextual details; it is neither historical nor developmental in its handling of materials; the data base used is static, thin, and disappointing.

Can a more favorable judgment rest on its theoretical contribution? The basic theory of fragmentation is said to be a flourishing body of knowledge, but "more accurate distinctions and more precise research are necessary before the theory of fragmentation can become fully useful" (p. 5). Only cursory discussion is made, however, either of the theory

itself or of the proposed augmentations thereto. Fragmentation theory appears to be an alias for pluralist theory, which is virtually equated with cross-cutting memberships and their benign effects. By contrast, fragmentation theory focuses on cumulative group memberships and their malign political consequences—the spiraling and destructive forces that can tear a community apart when its subcultures nourish feelings of in-group isolation and when intergroup antagonisms are projected into politics and even exacerbated by the political rivals who champion them.

Mr. Powell's strategy is to use theory to guide his exposition of the single case and to search for unanticipated features of the specific case to "inform" the development of the basic theory. By presenting "basic theory" in its "simplified version," complications do emerge requiring more sophisticated reformulations of that theory. It is difficult, of course, to find authors who can be quoted as sources for the "simplified version," so none are. Instead, in the first discussion, the sequence runs like this. Specific points actually made by Dahl and Verba are briefly examined. Next, reference is made to a line of argument that challenges "pluralist theory on other levels as well" (p. 4) by Converse and Lijphart. In 150 words, four working hypotheses are set forth for discussion. To round out the search for more incisive theory, it is then noted that social fragmentation probably will affect political conflict differentially, depending upon such undefined conditions as subculture awareness, political intensity, social segmentation, cumulative group affiliations, mass hostility, elite perceptions of mass attitudes, system instability, and system malperformance. None of these intervening factors is seriously examined, then or later, either theoretically or empirically, to clarify how they might qualify the link between fragmentation and hostility. Later excursions onto theoretical ice are similarly hard to pin down.

The poverty of his theoretical effort is as distressing as the flimsiness of his data base. Hostility—his key variable—is measured by two questions: distrust of the rival party should it come to power nationally, and/or aversion to cross-party marriages involving the respondent's family. Applied to Hallein, the dangerous implications of *lagerdenken* are difficult to trace. Even for partisans locked into a Socialist or a Catholic subcultural matrix of cumulative group isolation, only one-fourth worried about the rival party coming to power nationally. Fewer still were distressed at what might happen to their sons or daughters. In the sample as a whole, about one-eighth could be scored as politically hostile—at least on the functionally relevant plane of running the national government, where such a level might seem surprisingly low, given the rhetoric of modern politics. Still, partisans under cross-pressures were less likely than those under cumulative pressures to register hostility. With findings like these, since the author is able to assert that they "unambiguously confirm the hypothesis," who needs evidence (p. 39)? Six pages later, after failing to show that either class or religion alters the hostility levels among Hallein citizens, he argues that "these findings provide increased contemporary relevance for Marx's predictions of class conflict."

His closing suggestion is an understatement. "The nature of political hostility itself . . . should be subjected to a careful analysis, using more elaborate and more numerous attitudinal indicators than were used here" (p. 143).

The American Mafia: Genesis of a Legend. By Joseph L. Albini. New York: Appleton-Century-Crofts, Inc., 1971. Pp. xi+354. $3.95 (paper).

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Unfortunately, it usually takes as much time and energy to write a poor book as to write a good one. The lavish use of footnotes in this book suggests that Joseph L. Albini expended much time and energy on its preparation. He manages, in one way or another, to work in citations of most American books and articles on organized crime, including fast-buck pieces dashed off by free-lance writers. Further, a chapter on "The Mafia and the Camorra" parades familiarity with a portion of the some 200 books written about the Sicilian Mafia. Albini even reviews what some writers of undergraduate criminology textbooks have said about varieties of organized crime. (He misses the very insightful comments made by one such textbook writer—Walter C. Reckless.)

There is evidence, then, that Albini did not rush to press in an attempt to capitalize on the current popular interest in "Mafia books." Nevertheless, he has produced a bad book. It is pedantic without being scholarly It is not addressed to a general audience, but neither is it addressed to any discernible specialized one. It picks nits, but it also overgeneralizes. Its style clearly suggests that Albini does not know how to write, that the publisher's copyeditors gave the author no help at all, and that the editors of the publisher's Sociology Series (John F. Cuber and Alfred C. Clarke) asked for no manuscript beyond a rough draft.

Albini's basic objective seems to be that of knocking down something of a straw man, what he dubs the "*Evolutional-Centralizational Theory*" of crime syndicates. (He always puts it in upper and lowercase and italics.) At various points he says that this theory pictures syndicate structure in the United States as "highly organized," "rigidly organized," and "rigidly bureaucratized"; as a "nationwide unified criminal organization," and a "centrally directed organization"; as a "secret society," a "tightly knit society," a "centrally organized secret society," and a "national secret society"; and as an organization with "rigidly defined positions which are consistent among the national units, be they 'families,' 'cliques' or what have you."

Against such "rigid explanation" (p. 324) Albini sets what he regards as his contribution, namely a "*Developmental-Associational* approach." This device makes it possible to report a lot of what I can only characterize as yesterday's news. He says there are indeed "syndicates" that

have sold, are selling, and will sell demanded illegal goods and services in the United States; that these syndicates are "systems," "networks," and "a loosely formed system of relationships"; that they have "structure" and "function"; and that as "systems of power" they include "syndicate functionaries." The approach also enables Albini to say, again in very loose language, what others have earlier said more precisely. He announces that there is a "multitude of possible types of combinations of relationships among these syndicate participants," some of whom occupy "a middle position in a hierarchy" (p. 235), while others are "specialists" (p. 268), others are "individuals who have achieved the status of patron" (p. 316), and still others are "clients." Most important, Albini keeps repeating, is the observation, once again, that "syndicate participants" or "syndicate functionaries" engage in "patron-client relationships." He also seems to believe that the "*Developmental-Associational* conceptualization" produced his commonsense observation that a "patron" dealing with a "client" in, say, Chicago might have no clients at all in, say, Los Angeles; his mundane conclusion that an important criminal might be a "patron" here (or today) and a "client" there (or tomorrow); and his deduction, known even to the sleepy students, that an organization selling lottery tickets in Chicago must have a different division of labor than an organization smuggling narcotics from Turkey to New York.

Albini could have made an important contribution both to sociology and to a solution of "the organized crime problem" if he had given some attention to the sociologists' distinction between "organization" and "*an* organization," and then in some insightful way had spelled out the implications of this distinction for analysis of criminal conduct. Such analysis is difficult because any discussion of *a* syndicate, of *La* Cosa Nostra, or of *the* Military-Industrial Complex necessarily implies that each of these is *an* organization. Complete abandonment of the articles "a," "the," and so on, would lead to utter confusion and misunderstanding. Retaining them also leads to misunderstandings, especially when doing so is interpreted as evidence of belief in "rigid bureaucracy." Yet systematic manipulation of the articles—as in sociological discussions of "society" and "a society"—seems essential both to understanding criminal organization and to understanding organizations of criminals.

Albini leans toward analysis of organization rather than of an organization or organizations. But, apparently because he was unaware of the distinction, he is unable to say anything new or important about either phenomenon. He is mad at authors who assign names, especially Italian ones, to American crime syndicates, to the nationwide alliances between them (most evident in the "lay-off" betting procedure), and to the status positions, authority positions, and occupationally specialized positions making up the syndicates. This hostility might be justified if he had first demonstrated that there is organization but not organizations, or if he had shown that syndication can exist outside the structure of a syndicate. Missing his golden opportunity, he merely discusses the same syndicates, alliances, positions, and roles discussed by the persons he criticizes.

More specifically, Albini is quite indignant about authors who have found it reasonable to give the name "capo" or "lieutenant" to certain authority positions in each American syndicate, even if some of the persons occupying these "middle positions" do not call themselves "capo" or "lieutenant" and are given some other title by their associates. He could have documented any variation and then argued that it suggests organization or organizations rather than an organization. Instead, he joins the persons he regards as dumbbells. On page 316 he presents, in the "an organization" framework, a commonsense explanation of why there should be variation in the titles assigned to men who have achieved "the status of patron" both in Sicily and "among Italian-Sicilian syndicate functionaries located in various parts of the United States."

Perhaps Albini failed to follow up what appears to be his own hunches about organization because he got carried away by his obvious desire to be the first to speak the truth about crime syndicates. Perhaps it was this "set" that also made it impossible for him to try to build on, to confirm, or to prove erroneous any of the many hypotheses about such syndicates. And there are ways of checking some of these hypotheses For example, thousands of pages of bugged or wiretapped conversations between "syndicate functionaries" are now as available to sociologists as they are to newsmen, having been released to the public in connection with a New Jersey trial. Albini does not even refer to these documents, let alone analyze them. Instead, he erroneously claims that some similar and earlier documents contain only FBI summaries of criminals' conversations and comments. These Rhode Island documents, and the more recent ones, do contain FBI summaries. But both sets of documents also contain verbatim transcripts of conversations and comments—valuable data that are going begging, so far as sociological analysis is concerned.

Albini claims to have had good informants among both policemen and criminals, but he does not in a systematic way use any information acquired from them. Some of his allegations about crime syndicates are, to turn his own words on him, "so unfounded that they are not worthy of mention" (p. 246). His approach often involves merely fuzzing up the issue. On one issue, for example, he states a null hypothesis, although he does not call it that: "An examination of those who attended the Apalachin meeting hardly seems to indicate a meeting of a national organization." One would think that he would then present an "examination," thus testing the hypothesis. Instead of doing so, he changes the subject. After reviewing two sources that are secondary (or even more remote), he seems to accept as fact that the meeting was attended by at least one man each from New York, New Jersey, Pennsylvania, Texas, Colorado, Massachusetts, Ohio, California, Florida, Arizona, and Illinois. He concludes, "We find the bulk of those attending coming from the New York–New Jersey area rather than being a *representative* national group" (p. 249; the italics are mine). To be "national" must a group be "representative"?

The book contains an excellent 18-page bibliography.

Beyond Black or White: An Alternate to America. Edited by Vernon J. Dixon and Badi G. Foster. Boston: Little, Brown & Co., 1971. Pp. ix+140. $2.95 (paper).

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If black and white Americans are to find meaningful and workable alternatives to the present racial crisis, we must surely be careful not to base our change alternatives and strategies on the same racist philosophies that led to that crisis. Such is the thesis of this provocative little reader by Vernon J. Dixon and Badi G. Foster. *Beyond Black or White* is a refreshing departure from the empty rhetoric and speculation of the *Soul on Ice* and *Dark Ghetto* days, and it reflects a serious intellectual confidence which black scholar/activists might emulate. In gist, this talented assemblage of black scholars/activists from Princeton attempt to answer Dr. King's searing question: "Where do we go from here?"

The theoretical and problematic thread holding the book together is the contributors' view that the existing material on the racial crisis in America is mainly descriptive; and that in those few cases where strategies for change are offered, the thinking has been couched in the same spirit and philosophy which led to the present crisis. Essentially, the book is a coherent essay, and the intensity of the analyses reflects the contributors' ontological synchronism; that is, no polemics or theoretical clairvoyance—they all agreed from the beginning paragraph, and each demonstrated how Americans should best accept cultural diversity—beyond black or white!

The theme is cultural "*unity*" in spite (and because) of cultural "*diversity*" (italics mine; p. 4). The development of that position is organized under seven major headings which range from "Toward a Definition of Black Referents" to "The Dual Aesthetics of Black American Artists" and "An Alternate America." The overall frame of reference includes: (1) that black Americans, although embodying some cultural traits similar to those of whites, collectively embody a reference qualitatively different from that of white Americans; (2) racial conflict accrues from Americans' either-or conceptual approach to race relations (a black "gain" is perceived as a white "loss"); (3) black artists are subject to the influences of black and white Western culture; (4) economic institutions in America reflect the cultural foci of white (and not black) culture; (5) that restating the assumptions of American racism may thereby lead to a reformulation of strategies to combat that racism—black liberation; and (6) the "Diunital Approach" to an alternate America—an America where whites and blacks will acknowledge each other's differences and strive for the unity of their separate experiences.

Dixon's "Two Approaches to Black-White Relations" is, in my opinion, the most valuable single entry in the book. He reviews the literature very

well and offers the behavioral scientist a rather comprehensive model which he calls diunital—the state of being an element and not that element at the same time. Operationally, "We are at once both Black and not Black, we are this contradiction harmoniously" (p. 27). Dixon offers to students (who may need such clarification) an easy-to-follow typology on black organizations and includes a lively discussion on Hegel and Marx; and the progression to Fanon was welcome indeed. Dixon's other solo, "Diunital Approach to Economics," seemed to beg the question, however.

The major contribution of the book was Cross's "Psychology of Black Liberation." It was an academic snapshot of the nuances of the black psyche and the processes of "becoming" black. The book's antistrengths are its overreliance on unnecessary scholasticism, and it placed a disproportionate responsibility for the reality on "An Alternate America" on black people. Then too, it was a plug for cultural pluralism—without the attendant racist innuendos, of course. It was too short to be convincing, since it claimed to be an answer; yet I am sure the authors left acceptance or rejection of their thesis to the reader.

These brothers did not worry about value-free social science, and their book needs to be read religiously by graduate students and scholars in race relations seminars. For, all too often in those exercises, students are exposed only to questions and not to any answers. Moreover, the book shows clear and keen insight into the "black thing" as well as its understanding of Western white culture. Right on to Foster, Dixon, et al., they did us all a great service here. Such a work as theirs is invariably used as an auxiliary to "established" readers; it might be well that traditional works serve as amplifiers to this fine little work. I shall certainly use it in my own classes at this black institution.

Colonialism in Africa, 1870–1960. Vol. 3: *Profiles of Change: African Society and Colonial Rule.* Edited by Victor Turner. New York: Cambridge University Press, 1971. Pp. viii+455. $17.50.

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The task demanded of the 12 contributors to this book was truly herculean: to combine the small-scale field experience and social theory of anthropology with the written documentation and diachronic sense of historical research. It is not surprising that few have achieved their obtive. The greatest success seems to have come to those six authors whose essays conform to the apparent meaning of the volume title; they are "profiles" of particular African societies undergoing modern change. The most thoroughly worked out of these is Ronald Cohen's study of Bornu (Nigeria), which is mostly about precolonial history but develops a very full sense of the changes brought by European rule. The remaining

six contributions take up various general sociological themes, all relating to 20th-century European-African confrontations, if not necessarily using common definitions of colonialism. In some of this latter group the level of both theoretical bite and empirical substance leaves a good deal to be desired.

To deduce any general argument from the collection as a whole would be, as Turner himself concludes in the modest editor's introduction, "a fruitless project" (p. 4). The well-worn question of local political authority under colonial rule is treated in Cohen's chapter, as well as in those of John Middleton on the Lugbara (Uganda), Martin Klein on Sine-Saloum (Senegal), and Peter Rigby on the Gogo (Tanzania); surprisingly, it is largely ignored by the general essays. The authors writing on traditional authority agree that the "chiefs" of colonial Africa represented a break with previous political patterns. The degree of perceived change depended less upon European colonial policy (always cautiously interventionist) than upon traditional expectations. Bornu, a more resilient and highly centralized polity, experienced more continuity than the small-scale Sine-Saloum states or the totally acephalous Lugbara and Gogo. These conclusions would tend, indirectly, to support the provocative assertions in Max Gluckman's chapter that tropical Africa became, even after independence, more "pluralistic" than the Republic of South Africa. Pluralism is redefined by Gluckman to mean lack of political and social articulation, which he claims does exist in the oppressive South African situation largely as a function of widespread economic development.

Several essays in this volume treat colonialism in terms of "race relations," an approach which I have generally found sterile. Hilda Kuper, however, offers a precise and extremely useful account of the contrast between European and Swazi (Southern Africa) categories for perceiving alien peoples. Michael Banton, in "Urbanization and the Colour Line," presents some interesting descriptions and comparisons but offers them in a theoretical framework which seems either amorphous or tautological.

"The Impact of Imperialism upon Urban Development in Africa" is treated by Aiden Southall in a manner which sometimes approaches the polemical. Even where presented more detachedly, his views of how traditional African "cities" did or did not survive the European onslaught rests upon rather limited foundations both in urban sociology and African history.

Elizabeth Colson's contribution very neatly delineates how various European misconceptions of African land rights derived from the economic demands of paternalistic colonial situations. The essay would have benefited, however, from greater specificity in the references to "Earth Priests" and transitions to market agriculture.

Education and ideological change are the themes of chapters by Margaret Read on the Ngoni (Malawi), F. B. Welbourn on responses to missionaries, and Lucy Mair on new elites. Read's well-documented study is the most impressive of this group, although her optimism about Ngoni integration of traditional and European values seems at times a bit naïve.

Welbourn wanders over a wide range of varyingly convincing assertions which fail to do justice to his excellent original research on East African religion. Mair's essay covers the same ground as several more interesting pieces in the previous volume of this same series.

Language and Poverty: Perspectives on a Theme. Edited by Frederick Williams. Institute for Research on Poverty Monograph Series. Chicago: Markham Publishing Co., 1970. Pp. xii+459. $8.95.

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Language and Poverty explores the consequences of linguistic and cultural plurality for early childhood education. Current professional thinking has grown out of recent social changes and a new understanding of sociolinguistics (Roger Shuy, chap. 16) which, in turn is now helping to clarify some mechanisms of social injustice. But too often these authors do not carry their reasoning to its direct policy implications—they strongly resist going against established educational practice. The book is excellent as a basis for rethinking among teachers and related professionals, especially if discussion can go beyond the ideas in the book itself. It is also good as a case study of social bias and professional responsibility. The 19 chapters are well written, present various views on each topic, and include many references for further reading. The book concludes with an annotated bibliography of journal articles and a short topical index. There is one chapter each on American Indians (by Lynn Osborn) and on Spanish-speaking Americans (by Vera John and Vivian Horner), but the main emphasis is on black children.

The central thesis is that, while the United States has children from a variety of cultural and language backgrounds, its schools have not adjusted to this fact. Evaluation and testing of children's abilities has been heavily biased by preference for middle-class norms of response. The real abilities of children with other norms of response are ignored in the usual curriculum, and the lower expectations for these children by teachers and the children themselves become *self-fulfilling prophecies:* the child's self-confidence and motivation are affected and this *causes* failure. These ideas are best presented by Williams (chap. 1), Joan Baratz (chap. 2), William

Labov (chap. 9), Courtney Cazden (chap. 5), Ellis Olim (chap. 11, pp. 223–26), Davenport Plumer (chap. 14), and Williams (chap. 18). These chapters should be read first, as a foundation for the rest of the book.

Baratz presents the clearest example of test bias: on a special language repetition test designed for speakers of black English (BE), speakers of middle-class English (MCE) did at least as badly as BE speakers did on a MCE test (MCE is the reviewers' term). Standard tests, then, do not show that the typical black child has less language ability. He is not "deficient," "deprived," or "disadvantaged" in this regard. Instead, he knows a different, but equally "grammatical" (= regular) form of language, a dialect of English. The most flagrant form of this bias is in the Bereiter-Engelmann program (see Siegfried Engelmann, chap. 6), used notably in Head Start programs; if children do not respond with the exact words intended, they are assumed to have a language deficit and an impaired concept-forming ability! Other measures of scholastic ability (even IQ) are regularly misjudged in this way. "When measures not dependent on knowledge of the English language have been utilized, the Indian child has been found to perform within the national norms" (Osborn, p. 241).

More subtle bias arises from different social expectations in interview situations (Labov, pp. 157–63): the black child has learned to say as little as possible to be safe, especially when the questions are stupid because the answers are obvious. He does not interpret a question as a request for verbal display, as the middle-class child does; he communicates only necessary information and is then judged linguistically deficient. A reasonable conclusion would be that standardized testing methods cannot be used fairly across social and cultural cleavages (see Williams, p. 393). This point, however, is not really considered by Roger Severson and Kristin Guest (chap. 15, "Toward the Standardized Assessment of the Language of Disadvantaged Children").

Discounting myths based on past testing, David Yoder (chap. 19) admits that "the poverty child does not seem to be subject to more than the usual incidence of speech and hearing disorders found in the general population," but he goes on, "Although [this] would influence us against [a] 'speech correction' campaign with the poor, . . . it may not be safe to consider the issue closed, and we should wait for the results of such surveys as . . ." (p. 412). So presumably "speech correction" campaigns can proceed for a time, the children may be assumed guilty of deficit until proven otherwise, and neither Yoder nor Severson and Guest have adequately warned speech therapists and others that they may be responsible for damage to children. The USDHEW is far ahead of them when it has tried to restrict use of tests for ability-grouping of students.

Reading problems may result from linguistic and cultural differences (chapters by Baratz, Plumer, and Doris Entwisle): black children are being asked to read in a form of language other than their first dialect, and so of course display the symptoms of "poor readers." And not only do the phonetic forms and grammer differ: "In kindergarten and first

grade, word meanings, and therefore the cognitive role of words, overlap little for the black and the white child" (Entwisle, pp. 127, 136). So "cues that are presumed to exist, and hopefully aid the teaching of reading and other language skills, may be much more visible to some children than to others." Equality of opportunity requires *at least* teachers who are aware of these differences and can adapt their teaching to what the child brings (see Basil Bernstein, p. 57). Marion Blank (chap. 4) warns that despite our stated ideal of accepting differences among children, we tend to assume that all will learn in the same way, and that differential teaching is antidemocratic and implies illness on the part of those treated differently (pp. 72–73). Instructional materials must thus be translated culturally. This is a bare beginning.

Must they also be translated linguistically? One major obstacle is black parents who feel that the use of black-dialect texts will deny their children mastery of standard English. In clear foreign-language situations, teaching is most effective if it begins in the vernacular rather than in the standard. Children taught this way end up more competent even in the standard language (Osborn, p. 234; John and Horner, pp. 147–49). Of course, in a dialect situation, speech differences are not nearly as great. If the use of several dialects is encouraged in school, there should be few real difficulties of communication. Then special translations may be necessary only for initial reading texts.

The linguistically competent individual adjusts his form of speech *appropriately to various social contexts*, and *communicates* what he intends *effectively*; MCE is not always ideal in these terms. Labov argues "in many ways working-class speakers are more effective narrators, reasoners, and debaters than many middle-class speakers who temporize, qualify, and lose their argument in a mass of irrelevant detail" (esp. pp. 163–71). In fact, English teachers do fight excessive complexity in MCE. Francis Christensen (1968) argues against Kellogg Hunt's purported measures of "syntactic maturity": adding detail to a sentence with grammatically disconnected phrases may be more effective than using complicated subordinate clauses.

Bernstein (chap. 3) clarifies his "elaborated code" and "restricted code": they are *not* equivalent to linguistic proficiency and deficiency. But in a socially sensitive area he continues to assert causal links between social-psychological factors of home life and differences in language and communication behaviors, where in fact only correlations may exist (see Blank's warning, pp. 64–65; also chapters by Olim and Plumer).

Bernstein's description of linguistic factors is probably still faulty. First, "Language need only add what the context cannot communicate. Fragmented speech under such circumstances is efficient, not incomplete" (Williams, p. 394). Nonverbal communication is not "context"; it can be more appropriate than verbal communication and just as explicit. When one word can communicate a lot, this can hardly be called a "restricted code." Second, one may deal with either particulars or universals. The goal

of generalizing is to find "one integrated system which can generate order" (Bernstein, p. 52). This is appropriate to dealing with objects as means, but is inappropriate to dealing with persons as ends, where goals are not uniform. Perhaps formal middle-class language tends to promote a universalizing mode *too widely*. Olim links this to alienation and other current social problems. Third, elaborated explanation can be applied either to personal desires or to physical objects; but there is no reason to assume middle-class children have a superior reasoning power or concept of causality (see Labov, pp. 163–78; Cazden, pp. 89–90). Thus the doctrine of learning disabilities may, at least in part, be based on faulty reasoning. Abstract language is usually treated as desirable in educational contexts. Yet abstract words can sometimes convey less information than concrete words and can allow a dangerous distance between language and concrete experience, which encourages the manipulations of bureaucratese (Osgood 1971).

Editor Williams and several contributors emphasize the need for more research. By contrast, Plumer says: "In the last analysis it is the low social and political priority assigned to equality of opportunity . . . not the absence of adequate research—which is the chief obstacle to effective policy." For future research, we believe researchers are obliged to eliminate social bias more effectively, not only in their factual determinations but also in the questions they choose to ask. (Thus they should, on their own initiative, eliminate language-bias in testing, and should not assume MCE to be ideal.) Why for so long have we not evaluated teaching methods and schools just as critically as we have evaluated students to whom we attribute so many difficulties? Neither innate nor behaviorist positions (Harry Osser, chap. 13) grant the child an active role in enlarging his learning abilities. How to interest children is rarely discussed, though it is admitted as perhaps the most important goal (Plumer, p. 300; Labov, pp. 179–80).

Equal opportunity carries with it not only the acceptance of a multicultural society (Williams, p. 19) but even *limits on the legislation of culture*. If one language or culture is chosen for that of official affairs, including education at the lower levels, *the others should be offered compensation*. The language varieties grouped as "Black English" must be granted not only a long past history (William Steward, chap. 17) but also a future. Black English may show certain natural historical developments (such as loss of grammatical inflections) which MCE is also undergoing but has not yet taken as far. There are forms of BE parallel to other social and regional standards in the United States, and these forms must not be called "non-standard," as even Labov does. In fact the prestige of BE may be rising in New York (Labov, p. 273).

The elimination of linguistic variety may be not only impossible but also undesirable (Fishman 1966). Respect for diversity may be possible only when stereotypes are combated by personal interaction, and cultural differences are positively valued (Williams, pp. 396–97).

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In Britain and in America sociology has long stood for theory construction and social anthropology for the analysis of observations made in the field. Like all such distinctions this one is far from perfect, but it is largely true. David Mandelbaum's *Society in India* falls within this Anglo-American anthropological tradition. His view of social anthropology clearly is that of a cumulative science, and his conception of society that of empirically observable regularities in social interaction structured by cultural norms and values.

The scope of Mandelbaum's book is ambitious. He has set himself the task of assessing the extent of our empirical knowledge of some aspects of Indian society around 1968–69. A deeper and more significant aim is to attempt to grasp the nature of Indian unity that seems to underlie apparent diversities. The book "is intended as a general survey of basic social principles and patterns followed by most of the people of India. It is more a grammar of social relations than a dictionary of caste practices or a glossary of names" (p. 10). His method consists of examining empirical materials on microorganizations, collected and reported by hundreds of fieldworkers, in the light of the macrothemes of Indian civilization.

Society in India opens with a discussion, all too brief, of the ideological background of Indian society (chap. 2). It is not cultural norms and values, however, which so much emerge as the organizing framework for the examination of data as do some concepts derived from functional-structural-systemic theories of the types associated with British social anthropology and American sociology (chap. 1 and Appendix). The principal notions employed are those of "system" (interchange-counterchange), "structure" (ordered arrangement of relations), and "role" (goal-oriented, normatively defined behavior). "When some participants do not carry out the kind of *interchange* that others in the system anticipate, the others respond in regular ways of *counterchange* to restore some systemic regu-

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larity in their relations" (p. 5). Continuity and change are thus seen as functionally interlinked. The individual role occupant is described by Mandelbaum as "a manipulator of his culture and society as well as a creature and carrier of it" (p. 427). Such a theoretical perspective does not, of course, provide for drastic, sudden changes, but that would hardly seem to be a crippling disadvantage when one is dealing with India, particularly with rural India.

Mandelbaum focuses on village society in India, on its principal "groups," namely, the family (chaps. 3–7), lineage (chap. 8), *jati* (i.e., caste as commonly understood: Mandelbaum wisely avoids use of the word "caste," except as an adjective, because of its several referents) (chaps. 9–18), and village community (chaps. 19–22). The emphasis is on the structure of these "groups" and their interaction. Beyond the village and encompassing it lie Indian civilization and the forces making for modernization. Religion, economics, and politics enter the discussion only fleetingly. Their attenuation is defended thus: such an emphasis "would entail a different treatment. [Further t]his societal emphasis is made by many of the people themselves" (p. 10).

Change is identified as a familiar and recurrent process within Indian society though essentially subordinate to structure. Its typical manifestations are said to be mobility and adaptation within the caste system (chaps. 23–26) and, nowadays, politicization (chap. 27). Also discussed in the context of recurrent change are religious and tribal movements (chaps. 28–32). There is a fairly detailed discussion of tribal peoples and Muslims. The book concludes with a discussion of the nature of contemporary trends, some of which may turn out to systemic shifts rather than merely recurrent changes (chaps. 33–34). Thus, he observes, "The systemic changes that are in view do not point directly toward an unstratified society but toward fewer and more homogeneous social groups" (p. 634). Further, "People throughout India commonly keep to traditional social patterns while adapting them to modern circumstances. Abstract ideals are most readily revised; fundamental motifs of cognition and motivation seem little altered and are evident in the newer arenas of competition" (p. 655). Modernization is thus rightly seen as a process of juxtaposition rather than of displacement.

Mandelbaum draws his data from his own fieldwork among the Kotas (much less than one would have perhaps liked, considering the excellence of his publications on this people) and from almost 700 works—books, monographs, dissertations, papers—nearly all of them published, by various authors. The geographical distribution of the places and groups covered is widespread, though some areas are better represented (e.g., Mysore, Uttar Pradesh, West Bengal) than others (such as Assam, Bihar, and Rajasthan). He has also used materials from West Pakistan while discussing Muslim social structure. With a few exceptions the works on which *Society in India* is based are post-1950 publications. In other words, Mandelbaum has made a real and commendable effort to present us with an "all-India" picture which is contemporary. Judging by his extensive use of my mono-

graph on Kashmiri Brahmans (*Family and Kinship: A Study of the Pandits of Rural Kashmir* [New York: Asia Publishing House, 1966]), he seems to have taken great pains to employ other peoples' works without misrepresenting them. This is no mean achievement considering the length of the bibliography. The physical and mental resources required for such a task can well be imagined. Mandelbaum says that the idea of writing a book like this one occurred to him when he started his field research in India in 1937. Several drafts seem to have preceded the final version (see pp. vii ff.). *Society in India* may be, therefore, truly called the work of a lifetime. It will be a landmark in anthropological literature on India and is bound to be of immense value to those trying to get acquainted with the sociology of Indian castes, religious communities, and tribes.

It should be obvious that the strength of a work like Mandelbaum's must primarily lie in the availability of good materials. Thus, it is not at all surprising, for example, that the should have devoted greater attention to caste than to kinship, to the family than to the lineage, lineage-like categories, or the household. The differences in the attention that he gives to various topics, in fact, reflect the strength and weakness of Indian ethnography. Here it may be added that from among a few major works not considered by Mandelbaum the omission of books by Brij Raj Chauhan on Rajasthan (*A Rajasthan Village* [Delhi: National, 1966]); Ramkrishna Mukherjee on Bengal (*The Dynamics of Rural Society* [Berlin: Akademie Verlag, 1957], and *Six Villages of Bengal* [Bombay: Popular Prakashan, reprint ed., 1970]); and Dakfin Sivertsen on Tamil Nadu (*When Caste Barriers Fall* [London: Allen & Unwin, 1962]) seems grievous, as these would have helped him to add detail to some dimensions of village life, particularly political and economic.

Then there are the problems of ensuring the quality and comprehensive coverage of data. Often these two aims are in conflict. It is obvious that Mandelbaum has evaluated the available materials to make his selection of evidence, but we know this only indirectly. Within the book itself his concern is to piece together the evidence and not to pass judgment on it. Only occasionally does he permit himself to highlight areas and problems on which more fieldwork is called for. For example, he calls for more research on regional identification in India in an ever-widening circle of loyalties (see pp. 393 ff.). One wishes he had done this oftener: that would have greatly enhanced the value of his book.

As for comprehensive coverage, the task can be heartbreaking when one is dealing with a country as large as India: in Mandelbaum's own words, "The more one draws up, the more there is to draw" (p. 9). In other words, a survey like *Society in India* is bound to become outdated in a few years. Already there are indications that urbanism is no longer a negligible aspect of society in India. A considerable number of sociologists and anthropologists, including—to mention just a handful of names—Yogesh Atal, Richard Fox, M. S. Gore, Owen Lynch, Adrian Mayer, and Satish Saberwal are actively engaged in research in towns and cities, and their publications so far include several books and many papers.

Turning to a more serious problem, is Mandelbaum's method adequate for the purpose he has set before himself? He wants to grasp the unity that underlies social and cultural diversities in India. He seeks to do this by comparing empirical evidence drawn from different parts of India and by arriving at a lowest-common-factor picture. Inevitably, diversities get played down even when they are important as, for example, between north and south Indian kinship. Also, the limitations of the comparative method are not squarely faced. Thus, how far is it permissible to generalize on the basis of comparison between two castes in the same region—say, Brahmans and Rajputs in Rajasthan—or between the same caste (or *varna*) in different parts of India—say, the Brahmans of Tamil Nadu and Kashmir?

It seems to me that since diversities in India are an attribute of regional empirical systems, and are cultural rather than structural, the quest for unity must not be made in terms of them but in terms of some structural principles that are seen as fundamental to Indian civilization. Louis Dumont's attempt to discuss society in India (see his *Homo Hierarchicus: The Caste System and Its Implications* [Chicago: University of Chicago Press, 1970]) in terms of the basic principle of hierarchy is an example. Not that Dumont's approach is completely nonproblematic: the relation between ideology and the regularities in empirical behavior calls for careful consideration because of the discontinuity which characterizes it in certain contexts. Mandelbaum refers to this problem of discontinuity at several places in his book (see, e.g., pp. 628–29); and though he affirms at the very outset the relevance of ideology to empirical behavior, his book pushes it into the background—at least it seems so to me.

Finally a comment on the frills. The end-map giving the location of 97 villages, tribes, castes, etc., which feature prominently in the text, is useful, but it is a pity that it has not been included in the paperback edition which will naturally be used by more readers, particularly students. The bibliography is extensive, and serious omissions are few. The index, however, is far from adequate, in terms of the number, classification, and content of the entries. I have found more references to some entries in the text than are listed in the index. A few printing errors also seem to have crept in. One hopes these blemishes will be rectified in further printings of the book which are sure to follow.

Rural Credit in Western India, 1875–1930: Rural Credit and the Co-operative Movement in the Bombay Presidency. By I. J. Catanach. Berkeley: University of California Press, 1970. Pp. xi+269. $8.75.

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The role of rural credit in Indian agriculture is by no means straightforward. It is confused, on one hand, by virtue of frequently complex social and

political relationships which exist or develop between the parties involved, and on the other by an implicit interdependence of production and consumption decisions in peasant households. When cooperatives are considered as an additional structure for credit supply, the whole subject of rural credit and production is complicated by a new set of bureaucratic and ideological concerns; concerns which seem, at best, remote from the daily decisions of peasant households and agricultural production.

I. J. Catanach has provided us with some important insights and a well-documented account of the cooperative "movement" in the Bombay Presidency from its inception, following the Deccan Riots, up to the onset of the Great Depression. He promises us a critical examination of "first, the diagnosis of the rural credit situation in the Bombay Deccan which was offered in the years after 1875, and, then, the working . . . of the co-operative societies which were intended to provide a remedy for the supposed problem of agricultural indebtedness" (pp. 1–2). He hopes by this examination that the reader will be able to discover whether or not "the modesty of the success of co-operative societies in the Bombay Presidency [was] primarily the result of inertia within Indian society, or . . . the result of the essential unsuitability of the innovation offered" (p. 4). In his search for an answer to this question, the author deserves praise for an apparently very thorough examination of an immense number of official and unofficial documents and records relating to the establishment and encouragement of rural credit cooperatives in the Bombay Presidency.

We certainly agree with Catanach when he tells us that rural credit cooperatives were created by the state in what appears in retrospect to have been a genuine concern for rural welfare—in contrast to irrigation and transportation developments, seen by some as merely exploitative in intent Until their introduction in India, rural cooperatives had not been tried in a non-Western country. They appeared to their proponents in the early decades of the present century as a multipurpose weapon against a host of real and imagined ills plaguing India's rural economy. They held forth the promise of increasing—or at least mobilizing—rural thrift; and of relief to the agricultural debtor and the process of land transfer from agricultural classes to nonagricultural moneylenders which was thought to lie behind the riots of 1875. Credit cooperatives could link the interior to the credit sources of Bombay. They could serve as a nucleus about which the real or putative "village communities" might reemerge to serve the new needs of the growing nationalist movement.

As an account of what may be viewed as India's first serious encounter with rural development projects, Catanach's book is a valuable contribution. One cannot but be impressed by the dismal lack of understanding about the nature and regional variations in peasant agricultural organization which was manifest in the initial writings and policy recommendations of Bombay's English "guardians"—Harold Mann excepted. His description of Indian participation in both official and unofficial roles is no less interesting. To students of nationalist politics, caste movements, and administrative reforms, Catanach has illuminated an interesting area of

reference; but to the economist interested in the role of cooperatives as institutions for credit supply and as instruments of rural economic development, the book is deficient.

We read (p. 223) that cooperatives in 1930 supplied about two-fifths of the credit needs of some 10.7% of the province's rural population. The level of successful participation was much higher in such agronomically and culturally disparate districts as Dharwar, Broach, and East Khandesh, with 27.8%, 24.8%, and 18.7%, respectively, of the rural population involved. Unfortunately for the student of peasant history, Catanach's book does not go deeply enough into the reasons and events which lay behind the mixed picture of relative success and failure of rural cooperatives. Conditions of land tenure and weather risk provide ample explanations for some cases, but much greater detail is obviously needed for others. There are no detailed descriptions of the peasant economy of this period; neither is the reader given a much-needed description of how and by whom decisions were made at the level of the village cooperative society. It is perhaps unfair to criticize Catanach's book for these omissions, since he makes no pretentions of offering the reader detailed insights from the village level. Indeed, such insights would require both a measure of good luck and much painstaking labor at the local level. Nevertheless, if students of peasant history are to understand all the reasons behind the "modest success" of the cooperative movement in the Bombay Presidency before 1930, a considerable amount of work must be accomplished at the local level.

Anyone who is acquainted with post-Independence Indian rural development programs with their emphasis on cooperatives, community development, and reconstruction of ancient "democratic" institutions will find a certain sardonic note in Catanach's description of the cooperative movement before 1930. One suspects that peasant agriculture is still not well understood by those Indians and foreign advisers who would transform it into something better.

Singapore Population in Transition. By Saw Swee-Hock. Philadelphia: University of Pennsylvania Press, 1970. Pp. xiv+227. $12.50.

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This is the only major work published so far that covers so many aspects of Singapore's population growth for a period that extends back into the last century. The book, an enlarged version of the author's M.A. thesis completed in 1960 at the University of Malaya in Singapore (now University of Singapore), is largely a descriptive overview of Singapore's demographic trends from approximately 1870 to 1965. Chapter topics in this book are organized in the conventional way: historical background,

sources of demographic data, population growth and distribution, migration, population structure, fertility, mortality, labor force, family planning, and a concluding section on trends and prospects. However, there are two unconventional features, both of which deserve comment later on: the book does not have an introductory chapter but it contains a chapter on male working-life patterns.

Of course, the value of a book cannot be judged by its table of contents. Without an introductory chapter, the reader is left to himself to determine the conceptual scope of the book and what problems, if any, the author is attempting to bring to light. Having read the book, one has the distinct impression that this is essentially a compilation of what materials there exist on the demography of Singapore in the form of many tables and other statistics, accompanied by a straightforward description of the figures without much analysis either in a theoretical or policy-oriented framework. As such, it is difficult to say what the major thesis of the book is. Each chapter constitutes a highly compartmentalized unit in itself with little or no relation to others. Even in the 10-page concluding chapter ("Trends and Prospects") we found no evidence whatsoever of summarizing or integrating previous discussions. Instead, the last chapter (pp. 161–70) is devoted to a brief discourse on population and labor-force projections plus a three-paragraph discussion attempting to relate, rather incoherently, population pressure on development, growth of per capita income, the government's population policy, and the slackening rate of population growth.

The merit of this book lies in its bringing together into one volume much of the historical demographic data on Singapore with respect to fertility, mortality, and migration. We should be grateful to Saw Swee-Hock for assembling of data from sources which, as Irene Taeuber stated on the book jacket, are often "dispersed, fragmentary, defective to varying degrees, largely inaccessible outside specialized libraries." However, precisely because sources are diverse and data often defective, it is disappointing to find that the author has not added more footnotes to the text and tables indicating the sources of data and how he has tackled the problem of known data deficiency. In fact, with very few exceptions the tables and other statistics in the book show no source at all! The author of such a book is responsible for indicating sources of data so that others who have access to the same sources may be able to verify some of his arguments by reanalyzing the same data. One case in point concerns the age-specific fertility rates for the three main races in 1947 (p. 83). One is puzzled to find these rates because, according to the author (p. 17), statistics on live births by age and race of mother were first available only in 1952. There is no explanation in the book on how the author derived these rates. The puzzle becomes more acute when we observe that these rates are quite different from those given in a paper, "Fertility and the Increase of Population in Singapore," by You Poh Seng which, incidentally, the author fails to list in his bibliography. In any event, Saw Swee-Hock's figures are incorrect, at least inso-

far as he relates the live births of Singapore to the female population of the colony of Singapore, which in 1947 included other islands.

Another related point has to do with fertility changes in the prewar period. Basing his discussion only on the changing crude birthrates, the author is careful to emphasize that the increasing birthrates should not be readily interpreted as signifying an upward trend in fertility because of increasing normalization of the sex ratio of the population. Rightly so. One wonders, however, why the author chooses to examine only the crude birthrate when statistics on sex ratio are evidently available—evident from the author's own discussion of the changing ratios (pp. 58–62). Given the statistics, we can readily eliminate the distorting effect of the changing sex ratios by simply relating live births to the female population instead of the total population. As a matter of fact, we can even compute general fertility rates for the years, 1911, 1921, and 1931 because more detailed statistics are available from the reports of the censuses conducted in these three years. As the general fertility rates of all three main racial groups in Singapore showed significant increases between 1911 and 1931, we are more inclined to think that there had indeed been a genuine rise of fertility before the war.

The foregoing illustrations serve to indicate the lack of depth in what little analysis that the book provides. This can also be seen in the chapter on labor force, which is nothing more than a brief description of the 1957 census data, and the chapter on male working-life patterns, which comprises a series of simple computational procedures without any discussion of the results in relation to the structure of the labor force or the larger socioeconomic system. In the chapter on family planning and population control, the author simply summarizes the history of family planning associations in Singapore and outlines the essence of the government's first five-year program scheduled to begin in 1966.

Although the book was published in late 1970, almost all series of statistics in it do not extend beyond 1965. This is unfortunate, because a great deal more demographic data and analyses have become available since 1966 when a nationwide sample household survey was conducted More importantly, during the past several years Singapore has undergone tremendous social and economic changes. The implementation of substantial public housing and urban renewal programs and the rapid industrialization since 1965 have brought forth very significant changes in population distribution and labor-force structure. In the second half of the last decade, fertility continued to decline more sharply than the author's most optimistic assumptions in his population projections given in Appendix Three (p. 205). The government's first five-year family planning program has been completed, and more systematic evaluation of data will soon be available. In the light of all these developments and the publication of 1970 census results, those parts of the book which attempted to sketch a contemporary demographic view were already out of date when they appeared in print.

Until a better volume comes along, we would recommend this book to libraries and others interested in the historical demography of Singapore. One final note on the bibliography: while the author gives himself generous quotations on publications having to do with Malaya, several known articles directly relevant to the population of Singapore have been omitted, something which we hope is merely an oversight.

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